MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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SIR DAVID GILL, K.C.B., F.R.S., PRESIDENT, in the Chair.

Charles Edward Adams, M.Sc., A.I.A. (Lond.), Chief Computer, Survey Office, and Secretary, Surveyors' Board, Wellington, New Zealand;

Rev. David Ernest Llewellyn-Jones, The Vicarage, Maindee, Newport, Monmouthshire;

Percy Mayow Ryves, Farmer, 41 Miguel Servet, Zaragoza, Spain;

Rev. Frederick William Walker, Vicar of Ellerslie-cum-Epsom, Auckland, New Zealand; and

Hugh Williams, Lieut. R.N.R., Senior Assistant Marine Surveyor and Water Bailiff, Mersey Docks and Harbour Board, Halwyn, Cecil Road, Prenton, Birkenhead,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

Thomas Lawrence Bennett, M.A., Egyptian Survey Department, Cairo (proposed by F. J. M. Stratton);

William Heathcote Gilfillan, Surveyor-General of the Transvaal, Pretoria (proposed by R. T. A. Innes);

Rev. William Hall, R.N., Chaplain and Naval Instructor, Whitecote, Paignton, Devon (proposed by Percy L. H. Davis);

Charles Hope Harris, Surveyor-General's Office, Adelaide, South Australia (proposed by Sir Charles Todd);

James Hunter, Jun., Craigmore House, Craigmillar Park, Edinburgh (proposed by James Hunter);

Robert Howard Nolan MacDonald, B.A., Milleen, Dalkey, Co. Dublin, Ireland (proposed by E. T. Whittaker);

Henry Lloyd Meadows, M.A., Clerk of the Crown and Peace for County Wexford, Ballyrane, Killinick, Co. Wexford, Ireland (proposed by Sir Howard Grubb);

Bert Perrott, F.C.S., Science Lecturer, Bryn Clydach, Neath Abbey, Neath, Glamorgan (proposed by Charles R. Davidson);

Rev. Edmund Sixtus Phillips, Professor of Science, Oscott College, Birmingham (proposed by the Rev. A. L. Cortie);

Abraham Barsby Taylor, 33 Lansdowne Road, Aylestone Park, Leicester (proposed by T. D. Brewin);

Hari Prasad Vidyant, M.A., C.E., F.C.S., Executive Engineer, Public Works Department, Makbulganj, Lucknow, India (proposed by Phanindra Lal Gangooly);

James M. Williams, F.R.G.S., F.L.S., Duffryn, Parkstone Avenue, Canford Cliffs, Bournemouth (proposed by J. Netherclift Jutsum); and

William Matthew Worssell, Assistant, Transvaal Observatory, Johannesburg, S. Africa (proposed by R. T. A. Innes).

One hundred and seventy-eight presents were announced as having been received since the last meeting, including, among others:—

Royal Observatory, Greenwich, Second nine year Catalogue for 1900·0, presented by the Observatory; G. F. Chambers, The Story of the Comets; George Forbes, History of Astronomy; Sir William Huggins, Collected Scientific Papers; F. K. McClean, Report of the Solar Eclipse Expedition to Flint Island, January 3, 1908, presented by the authors.

Astrographic Chart; 90 charts, presented by the Royal Observatory, Greenwich; 20 charts, presented by the Tacubaya Observatory.

Photographs of the Spectra of Meteors, presented by Professor E. C. Pickering; Photographs of Mars taken through screens in the focus of the 30-inch refractor of the Poulkovo Observatory, presented by M. Tikhoff.

On an Error in the New Lunar Theory.

By Ernest W. Brown, M.A., Sc.D., F.R.S.

Two different constants were accidentally used in the numerical results for the terms due to the figure of the Earth in my lunar theory (*Mem. R.A.S.*, vol. lix. pp. 79, 80). Hansen's observational value was used in the additions to the mean motions, while Faye's was used in the remainder of the terms. They can be made consistent by the following changes:—On page 79, lines 7, 6 from the bottom, change (β) into (α), and "Hansen's" into "Faye's"; and on page 80, line 5, change $641''\ t_c$ into $657''\ t_c$ and $-600''\ t_c$ into $-615''\ t_c$. The same changes in the numbers should also be made on page 94. Also on p. 455 of volume lxviii. of the *Monthly Notices* it is stated that the constant used corresponds to an ellipticity 1/296·3; the number should be 1/292·9, and in the footnote (α) should replace (β).

The matter is of some importance, because the principal terms due to the oblateness of the Earth should ultimately furnish by comparison with observation perhaps the most accurate method for the determination of the difference between the equatorial and polar radii of gyration of the Earth's mass. It is immaterial at the moment which constant is used for the preliminary tables of the Moon's motion, since it will have to be corrected by comparison with observation. Of course it is essential that all these results of the theory should have the same constant.

I am indebted to Dr. Frank E. Ross for pointing out the error.

New Haven, Conn.:
1909 *October* 20.

Note on the time of Perihelion Passage of Halley's Comet.

By P. H. Cowell, M.A., F.R.S., and A. C. D. Crommelin, B.A.

(*Communicated by the Astronomer Royal.*)

Taking the elements of the orbit as follows,—

$$\left.\begin{aligned} \omega &= 111^\circ\ 42'\ 16'' \\ \Omega &= 57\ 16\ 12 \\ i &= 162\ 12\ 42 \end{aligned}\right\} 1910{\cdot}0$$

$$e = 0{\cdot}967281$$

$$\log a = 1{\cdot}2539958$$

$$\log q = 9{\cdot}7687858$$

—tabular places were computed with different values of T, and by interpolation the values of T were found which would produce exact accordance with the Right Ascensions and Declinations of the comet determined from the Greenwich photographs of 1909

September 9, October 6, October 19, November 5. The resulting values of T are—

Date of Observation.	Deduced Value of T.	
1909.	From R.A.	From Dec.
Sept. 9	1910 Apr. 19·738 G.M.T.	19·648 G.M.T.
Oct. 6	19·726 ,,	19·650 ,,
,, 19	19·719 ,,	19·645 ,,
Nov. 5	19·712 ,,	19·648 ,,

The fair agreement between the two columns indicates that the errors of the assumed values of Ω and i are small. The progressive change indicates a true value somewhat less than April 19·7. Taking it as April 19·65, and the other elements as above, a nearly central transit of the comet over the Sun results on 1910 May $18^d\ 14^h$, visible in the Pacific Ocean, and in Asia, Australia, etc. Its observation is desirable, as likely to afford evidence of the constitution of the denser parts of the comet. Even if the comet proved to be totally invisible on the Sun, this would give evidence of a negative character. Photography with the spectro-heliograph may be suggested, also ordinary photography with very short exposures, as possible methods of detecting the comet's presence. The comet will traverse the Sun's disc from west to east, and will take about 1 hour in crossing a diameter of the Sun.

Royal Observatory, Greenwich:
1909 *November* 12.

On Correlation and the Characters of Variable Stars: a second reply to Professor Karl Pearson. By H. C. Plummer, M.A.

1. I am very grateful to Professor Pearson for his extremely courteous and temperate reply on this subject.* It has not led to any material change in my views, for the criticism was not made lightly, and cannot be lightly abandoned. But it gives an opportunity of defining our differences more clearly; and possibly, it may be hoped, of removing some misunderstandings.

Let me assure Professor Pearson that he has misconceived the bearing of the final sentence of my criticism when he reads it as my description of his paper. The conclusion had been reluctantly arrived at, whether rightly or wrongly, that the application of statistics to photometric data alone would not lead us very far or very surely in the direction of a knowledge of the physical processes operating in variable stars. Hence in the final paragraph a policy of concentration on the observing side was advocated as a more hopeful alternative, and the last sentence about "mere accumulation," etc., had no special reference to Professor Pearson's

* The references are to *Monthly Notices*, vol. lxix.

paper at all. Not only would the phrase have expressed a disrespect which was wholly unintended, but it would also have been inappropriate, inasmuch as Professor Pearson had availed himself, without criticism, of material which had been collected by others.

So far as the results in the original paper are claimed as the outcome of modern statistical theory, they are naturally subject to two tests: (1) Do they give us new and useful information which would otherwise have escaped us, and which will serve as a fertile source of suggestion for further work? (2) Does the method of derivation give them a rigorous certainty which would otherwise be lacking? These were the points which guided my criticism, and on which I feel no greater confidence after reading the rejoinder.

2. There is one matter about which it can only be supposed that Professor Pearson has misunderstood my meaning. It was suggested that a very large value of $r(L_2 - L_1, L_2)$ could be accounted for by the fact that L_1 is very small in comparison with L_2. Of course this is not a universal truth, but most intelligent readers would understand its meaning perfectly and acquit me of any statistical heresy. It is true, and only true, if the smallness of the second quantity implies necessarily a smaller standard deviation; and this is frequently the case. May I be forgiven the following illustration? Let us consider this problem: Is the size of a parasite correlated with the size of its host? Suppose that 100 fleas were taken from as many dogs of different breeds and sizes. Let each dog be weighed and also the flea taken from it. The answer to our question would depend on correlating the two series of weights. But without making the experiment at all it is quite certain that the coefficient of correlation between the weights of the dogs and the differences between the weight of a dog and the weight of the corresponding flea would be near unity, and the magnitude of the coefficient would teach us nothing.

The view that Professor Pearson has altogether misunderstood my point is confirmed by the fact that the numerical example (p. 575) which he has given is a simple and apt illustration of my contention. A third case is added which will illustrate the contrary state of things:—

I.	$x=$	300,	400,	500,	600,	700,	800,	350,	450,	550,	650
	$z=$	3,	4,	5,	6,	7,	8,	3·5,	4·5,	5·5,	6·5
II.	$x=$	300,	400,	500,	600,	700,	800,	350,	450,	550,	650
	$z=$	5·5,	7,	5,	8,	4,	6·5,	3·5,	4·5,	6,	3
III.	$x=$	93,	94,	95,	96,	97,	98,	93·5,	94·5,	95·5,	96·5
	$z=$	5·5,	7,	5,	8,	4,	6·5,	3·5,	4·5,	6,	3

In case I., x and $x-z$ are completely correlated, $x-z=\cdot 99x$ and $r(x-z, x)=1$. In case II., the order of z has been deranged, apparently at random, and there is nothing to suggest any relation between the two sets; nevertheless it is quite certain, even without

making the calculation, that $r(x-z, x)$ is at least ·99995. Finally, case III. illustrates a condition which I had by no means overlooked, as Professor Pearson supposes, where $r(x-z, x)$ may be zero in spite of the fact that z is small in comparison with x.*

3. Now in the case of the long-period variables need we go further to see that the application of the theory of correlation has not led to conclusions which are at the same time necessary and useful? What is to enable us to distinguish the nature of the conditions in this class of stars from the case II. above? It does not mean that we are denying the correlation between $L_2 - L_1$ and L_2 in a sense, any more than that we are questioning the accuracy of Miss Bell's arithmetic. But the question is whether the figure obtained for $r(L_2 - L_1, L_2)$ does really tell us more than we knew already, namely, that the majority of the stars in the selected list vary by at least two magnitudes. It is apparently a natural consequence of this obvious fact that $\sigma(L_1)$ is very much smaller than $\sigma(L_2)$, and from this again it must follow at once that $r(L_2 - L_1, L_2)$ is large. But this does not bring to light any organic relationship, and it is difficult to see the advantage of translating a fact already obvious into the much more obscure language of statistics.

But Professor Pearson claims the merit of bringing out the exceptional cases. Here again the essential fact can be stated in ordinary language as a simple deduction from the data without any appeal to diagrams or calculations. The majority of the stars in question vary by at least two magnitudes, *but a few* (roughly 10 per cent.) *vary by less.* Or, in saying this, are we overlooking some important fact? Curiously enough, if a star were discovered which varied by 100 magnitudes, it would not be exceptional according to Professor Pearson's criteria. Surely the interesting fact here is that the cases of small variation among long-period stars are few. But all that we know is that the discovered cases are few. The difficulty of ascertaining the variability of a faint star with small range and long period must be exceedingly great, and it may be enough to account for the rare occurrence of such cases.

4. It is now necessary to explain the sense in which I used the word "spurious," as this seems to have given rise to misunderstanding, and is connected with one of the objections of Professor Pearson. If we have simultaneously, except for some superposed element of variation,

$$x = az, \quad x - z = bx, \quad x - z = cz,$$

we have, as I suppose, a case of genuine correlation which applies to any pair of x, z and $x - z$. But if we have to suppose one equation true and the others untrue, there appears to be a difficulty. Except in the special case when $x = z$ it is difficult to estimate the value of an equation to which we must not apply the simple rules of algebra. Now of correlations in such a case may

* The point (*b*) § 3 (p. 576), if taken literally as expressed, is so far removed from anything suggested in my paper that it needs no answer.

we not say that they possess a spurious element, because they correspond to results which are only true in a limited sense? The correlation is right as far as it goes, but naturally we get into difficulties when we try to make deductions.

And here the correlation triangle appears to give us a useful warning. We may calculate a correlation coefficient by the formula

$$r(x, x-z) = \{\sigma^2(x) + \sigma^2(x-z) - \sigma^2(z)\}/2\sigma(x)\sigma(x-z),$$

and its size may lead us to expect a genuine relation. But if we find that it scarcely exceeds the limit

$$\sqrt{\{\sigma^2(x) - \sigma^2(z)\}}/\sigma(x),$$

which involves only two instead of all three standard deviations, we must, I think, be prepared to modify our expectation. Under the circumstances it seems right to argue that the significant thing is the small ratio $\sigma(z)/\sigma(x)$, and not the large coefficient $r(x, x-z)$, because the former will account for the latter, but not the latter for the former. In such a case we may expect the correlation to be affected by a spurious element in the sense just explained; but I have never suggested, as Professor Pearson implies, that the simple difference between the coefficient and the limiting value is the true measure of the genuine correlation.

In passing, it may be noted that Professor Pearson is wrong when he deduces from my triangle that $r(x, z)$ and $r(z, z-x)$ both approach zero when $r(x, x-z)$ is nearing unity. This would mean $B = C = 90^\circ$ when $A = 0$. What is true is that their sum approaches zero, but each may be numerically unity, as he will easily convince himself if he will give the triangle a second glance. The Algol stars happen to be a case in point.

5. We must now turn to Professor Pearson's example drawn from the measurements of prehistoric femora to which he seems to attribute a generality which is possibly excessive. Let us compare it with the parasite example already proposed in § 2. We may call the length of the right femur R, of the left femur L, the weight of a dog D, and the weight of a flea F. Now let us take together the sets of quantities which occur in the two illustrations—

$$\left.\begin{matrix} R \\ D \end{matrix}\right\} \qquad \left.\begin{matrix} L \\ D-F \end{matrix}\right\} \qquad \left.\begin{matrix} R-L \\ F \end{matrix}\right\}$$

Remembering that statistics, as such, are concerned merely with numbers, and not with things, what is the difference between the two examples? There is only one, as far as I can see, and that not material: it is that R − L may be negative in a few cases, while F is necessarily positive. The parasite problem is a perfectly reasonable one, and typical of a large class; *e.g.* the relations between the height of a tree and the length of its seed, the height of a man and the width of his front teeth, and so on. Now what would Professor Pearson think of a man who computed the value

of $r(D, D-F)$, found it to come out large, and announced the result as a scientific discovery? Whatever the particular figure found, our knowledge of the real problem would not be advanced in the very least. Is it very different in the case of the femora? We know very well that the majority of mankind do not hobble. A mere glance at the prehistoric skeletons will reveal the same fundamental fact. The large value found for $r(R, L)$ only expresses the same fact in different language. But it tells us practically nothing about any true physical law as to the variations to which the two limbs may be subject. And yet would not that be precisely the problem which would concern the anatomist?

It will be noticed that, so far as these illustrations go—and illustrations by themselves can show very little—they refer to cases in which two quantities approach a ratio of equality. May it be suggested that in these cases the relative order of the three standard deviations conveys the desired information more directly than the correlation coefficients? Thus, in the example of the femora, not only does the ratio $\sigma(R-L)/\sigma(R)$ imply that $r(R, L)$ must be at least ·9615 (against ·9618), but the ratio $\sigma(L)/\sigma(R)$ implies that $r(R, R-L)$ must be ·3310 (against ·3357), while the same test cannot be applied to $r(L, R-L)$ which comes out only −·0657. Surely the interesting thing is, not that the femora are correlated, but that the variability of the right femur is greater than the variability of the left femur, because this is not *a priori* obvious, and that the variability of the difference is much less than either, though this might reasonably be anticipated. And is not the former of these remarks connected with the still simpler fact that, on the average, the right femur is longer than the left? This, however, is a question for an anatomist, not for an astronomer.

The suggestion that the ratio of the standard deviations is the important thing, which the consideration of a particular type of illustration has led us to without forethought, has a bearing on a question which one has heard asked: Does the modern theory of correlation give us information which would have escaped us if we had only been armed with the old theory of least squares? Far be it from me to generalise from a single type of illustration, though it is proposed by Professor Pearson. But so far as this example goes it does seem that the standard deviations give us immediately all that we are likely to discover. Now standard deviation is only the modern name, more appropriate in its extended sphere, for our old friend the error of mean square or mean error. But of course the ratios of the standard deviations and the correlation coefficients are mutually convertible, and I have no wish to disparage even this part of the modern theory, while for other parts I have the greatest admiration.

At the same time it is not pretended that the question of the contrasted illustrations is disposed of by the above considerations, which isolate the particular problem and make abstraction of the physical character of the entities involved. Professor Pearson

may exercise the right to choose his own primary variables—in other words, to correlate everything. He may in this way correlate every pair of bones in the skeleton, and thus arrange them in a series which will show a progressive intensity of organic relation. The results can only be appreciated and criticised by an anatomist. They may well be of the highest value. But it is claimed that a knowledge of the nature of the material to which a statistical treatment is applied is still necessary, and that the statistician's results in every case must be justified, and can only be justified, by their utility.

6. Professor Pearson apparently objects to my correlation triangle altogether. May it be pointed out that it is he, and not I, who formed the triangle when he calculated the three standard deviations and the three correlation coefficients? I am merely responsible for the trifling and obvious remark that these numbers are related in the same way as a, b, c, cos A, cos B, cos C. When he says that he chose the largest coefficient partly because it was the largest coefficient, it is only expressing the same thing in other words to say that he chose the smallest angle of a triangle partly because it was the smallest angle. But then there is little reason to be surprised if its cosine is a considerable fraction.

He also objects to the application of the triangle indifferently to the three types of variable stars. To judge of the use to which it was put, the general argument may be resumed thus: We have a series of maximum magnitudes m_2 and a corresponding series of minimum magnitudes m_1. Professor Pearson assumes the existence of some common relation operating in each star: briefly, my whole contention is that the existence of such a relation has not been demonstrated. There can be no question that m_1 and m_2 are the primary data of the problem. His first step is a change of variables from m_1, m_2 to L_1, L_2. Then he finds $r(L_2 - L_1, L_2)$ to be large, and considers the magnitude of the coefficient to have physical significance. Does this not mean some relation between the physical circumstances of each star at maximum and minimum? But m_1 and m_2 are the only indicators we have of these different circumstances. Hence any demonstrable relation between the two states must exist in the form of a bond connecting m_1 and m_2. But now comes the simple argument: Let all the bonds to which we are attaching importance be broken by completely deranging the order in the two series of magnitudes, and let the correlation coefficient to which significance has been attached be recalculated. What happens? We find that the utmost ingenuity cannot appreciably depress the value of the coefficient below the figure already obtained. May we not infer that the bond, the strength of which we are trying to measure, cannot even be proved to exist by the method adopted? That is the simple argument which was put forward, and nothing in Professor Pearson's rejoinder seems to affect it.

7. May it not be suggested then that the value of the formal results of the statistician depends on the concrete deductions which

can be drawn from them? When Professor Pearson declines to be "dogmatic about a single conclusion" in his memoir, he makes all criticism seem unnecessary and unfair even when it is inspired by a desire equal to his own to arrive at truth in as certain a form as we may. But then elsewhere he claims to have enabled us in the case of the Algol stars for the first time to predict $L_2 - L_1$, L_1 or L_2 from a knowledge of any one of them. What does this come to? The relation he has given us is practically (p. 141)—

$$L_2 - L_1 = \cdot 64\ L_2,$$

supported by a high correlation. The unwary would conclude that—

$$F = (L_2 - L_1)/L_2 = \cdot 64,$$

or the fluctuation is a given constant in this class of stars. But nothing of the sort. The fluctuations of the dozen stars with a period less than one day are ·31, ·43 (two), ·48 (three), ·52 (two), ·56, ·60, ·64, ·84. May it not be suggested that a claim of this sort does a real injustice to those valuable developments of statistical theory which we owe to Professor Pearson?

Not that it is pretended that this case of the Algol stars is free from perplexities. On the one hand, the claim referred to above seems too high. It is not only that the accuracy of prediction is rather rough even in the case of the stars actually discussed, but, in common with all classes of variable stars, there is no assurance that we are dealing with a random selection. The important point which Professor Pearson consistently ignores is that stars whose fluctuations are small are necessarily omitted simply because we have no means of discovering them. On the other hand, there need not be any objection to the primary variables chosen, and the requirement made in § 4 above is satisfied, although this in itself is not enough for complete correlation: it is not only necessary that the three relations shall coexist, but that they shall be consistent. Moreover, though Professor Pearson objects to my proposed limit altogether, it may still be not unworthy of notice that the correlation does exceed the limit quite sensibly when the Algol stars are compared with the other classes in this respect. On the whole, it is not impossible that some relation is effective here, and may be discovered ultimately, though I cannot admit that it has yet been put in a useful form.

A very careful reader of my criticism might have noticed that on this point more guarded language was used in the case of the Algol stars than of the other types. The same applies to the reference to the correlation between the fluctuation and the period for this type. I am sorry that I missed the connection of a few words at the top of p. 142. But, apart from this slight oversight, we are in this instance in agreement, though Professor Pearson arrives at his conclusion by a more devious path. There was no intention of denying that a positive correlation between fluctuation

and period was indicated, though the material is hardly sufficient to make us feel quite sure. Hence an alternative suggestion to explain the result seemed quite in order, but it is too small a matter to be dignified as a "rival theory."

8. Since the maximum and minimum magnitudes are the sole data of the problem on which we are so completely at variance, it is difficult to understand the bearing of those considerations as to the distances of the stars which plainly dominate Professor Pearson's line of thought so completely. In particular, how can the cluster variables be affected, seeing that they can safely be assumed to be situated at one common distance? And in the case of the Algol stars, how can the effects of distance and intrinsic brightness be disentangled when we are equally ignorant of both? It seems to me that we can only deal with variability as a resultant of all the many factors involved.

I am not much concerned to defend the result of correlating m_2 and $m_2 - m_1$ for the cluster variables. The computation was made out of curiosity, and having been made was included in my paper. So far as it goes, it points to an empirical relation between m_2 and $m_2 - m_1$, and as a matter of fact it is only necessary to plot these numbers to see that a linear connection is quite plainly indicated. Even if it is insisted that the result has no physical meaning, the empirical fact remains and may even be useful. Why may one not insist on the right to assume these quantities as the primary variables, and turn the whole of Professor Pearson's argument (§ 7 of the rejoinder) into an elaborate *tu quoque*? But if m_2 is a number with a complex origin, while $m_2 - m_1$ is a number of simpler character, is that a fatal objection? If one must choose variables with the same physical unit, how comes it that Professor Pearson has correlated light-fluctuation with period?

But he does not appreciate the reasons which led me to put in a word for the use of magnitudes. In the first place, they are the primary data, and what we have ultimately to explain is the association of m_1 and m_2. If we use functions of our data, the utmost care will be necessary to make sure that we are not introducing correlation by our very procedure, especially when we are dealing with quantities which are obviously conditioned as in this case. Both m_1 and m_2 are positive quantities within a limited range; m_1 is greater than m_2, and $m_1 - m_2$ exceeds a limit which is set by the attainable accuracy of our observations, and in a measure by the assiduity and luck of the observer. In the second place, the astronomer ordinarily uses magnitudes for two reasons. One is simply the force of tradition, but the other lies in their arithmetical convenience. With no more than three figures we have, as a rule, an adequate measure of the brilliancy of all the heavenly bodies. The magnitude of the Sun is about -26, of the brightest star about -1, and an equal step to 24 will carry us beyond, but perhaps not very far beyond, the faintest star of which the most powerful modern instrument will make us aware. Professor Pearson himself can hardly have failed to become conscious of

the inconvenience, from the practical point of view, in using luminosities; the effect of three exceptionally bright stars in the Algol group is a case in point.

But can one deny the danger alluded to above, which follows from the transformation of variables under actual conditions? On this point let me invite attention to the example III. in the preceding § 2. The numbers have been written down practically at random. We may suppose $r(x-z,\ x)$ not extremely large, at all events; it may even be small, though it is not worth while to find out, because the actual figures are purely illustrative. Let us make the transformation—

$$X = 10^x, \quad Y = 10^y.$$

Merely as a result of the transformation, the deviations of X from the mean become enormous in comparison with the deviations of Y. We start with quite disconnected numbers, and yet we shall find if we calculate $r(X,\ X-Y)$ that to express the result by a figure differing from unity at least 180 decimal places will be necessary. Can it be felt that a word of caution on this point is entirely superfluous?

1909 *June* 9.

The Magnetic Storm of September 25, 1909, and the associated Solar Disturbance. By William J. S. Lockyer, M.A., Ph.D. (Plates 1, 2.)

The probable association between magnetic storms and active conditions on the solar surface renders it of importance to gain as much information as possible on this very interesting subject. The recent occurrence of a large magnetic storm with accompanying aurora, and its suggested connection with the group of spots that was situated near the central meridian of the Sun on that date, is a case where facts should be collected and co-ordinated.

The object of this communication is to present some data which have been obtained from observations made at the Solar Physics Observatory, South Kensington.

It may, in the first instance, be mentioned that the magnetic storm in question began, according to the Kew records, at 11.43 a.m. on September 25, and terminated at about 8.30 p.m. on the same day. That it was of considerable magnitude was shown by Dr. Chree's statement* that "the range of the elements has probably not been exceeded during the last twenty years, not even during the great storm of February 13, 1892."

Monsieur Angot† has also stated that it was the most intense that has been recorded since the commencement of magnetic observations at Parc-Saint-Maur, that is to say, since 1883.

* *Nature*, vol. lxxxi. p. 396.
† *Comptes Rendus*, vol. cxlix. p. 577.

According to Father Sidgreaves,* the storm was at its height between 3.30 p.m. and 5.30 p.m. on the 25th, so one may take 4.30 p.m. as approximately the epoch of maximum intensity.

Since the year 1903 (July) routine work has been carried on at the Solar Physics Observatory with the spectroheliograph in photographing the disc and limb of the Sun in monochromatic "K" (calcium) light. Unfortunately, on September 25 last no records could be secured with this instrument owing to cloudy weather, but on the previous day (September 24) four photographs of the disc and one photograph of the limb were obtained. Each of the photographs of the disc shows, in addition to small flocculi, three large groups of flocculi.

Attention is here specially drawn to the tremendous activity which was displayed in the flocculus associated with the spot (approximate latitude 5° S., longitude 305°) near the central meridian, an activity which is quite unique in the records made with the spectroheliograph of this observatory.

Fortunately the weather was sufficiently favourable prior to the 24th to inquire into the previous history of this group, photographs of the disc being secured with the spectroheliograph on the 17th, 18th, 20th, 21st, and 22nd. On the first of these the spot group is on the limb.

All the photographs of which mention is here made were taken in the manner usually adopted at this Observatory by adjusting the secondary slit on "K_2," the central portion of the "K" (calcium) line. The eight photographs (enlarged 3·5 times) here illustrated (that of the 17th being omitted) were exposed at the dates and times given in the following table, which contains also the length of time (in seconds) taken for the primary slit to travel across the solar image.

		G.M.T. h m s	
No. 1	Sept. 18	10 28 36	52 seconds
2	20	9 58 30	34
3	21	12 12 10	50
4	22	10 42 2	90
5	24	10 5 50	80
6	24	10 10 59	80
7	24	11 11 17	65
8	24	11 16 24	65

Description of the Photographs.

The photograph (not here reproduced) secured on September 17 at $11^h\ 50^m\ 26^s$ shows the flocculus group, to which special attention is called in this paper, on the eastern limb of the Sun. It appears

* *Nature*, vol. lxxxi. p. 426.

as a bright crescent, with an intense small brilliant point in the middle, this portion partly projecting from the limb. The intensity here displayed is unusual, and has not the appearance of normal flocculus on the limb.

September 18, $10^h\ 28^m\ 36^s$.—On this day the group is further round, and it can be seen as a whole. It has the appearance of two umbræ separated by an unusual bright streak, the larger umbra being on the western side (Plate 1, No. 1). The calcium flocculus surrounding the umbræ appears of normal intensity, but very much fainter than the bright streak, the latter having the appearance of an overhanging prominence.

September 19.—Sunday. No record.

September 20, $9^h\ 58^m\ 30^s$ (Plate 1, No. 2).—The group is here better exposed to view, but there is nothing very striking about its appearance to make it differ from ordinary calcium photographs of spot regions. The umbra is here triple.

September 21, $12^h\ 12^m\ 10^s$ (Plate 1, No. 3).—The greatest change from the previous day is the increase of intensity of the flocculus region lying immediately to the west of the western umbra, and taking the same curvature. It is interesting to compare, in this series of photographs, the intensity of the whole flocculus group with that lying towards the south-west.

September 22, $10^h\ 42^m\ 2^s$ (plate 1, No. 4).—The configuration of the group does not differ very much from that of the previous day. The chief change is, however, in the intensity of the flocculus to the west of the western umbra. Its intensity has considerably increased, and this is apparent far more in the original negative than is shown in the accompanying reproduction.

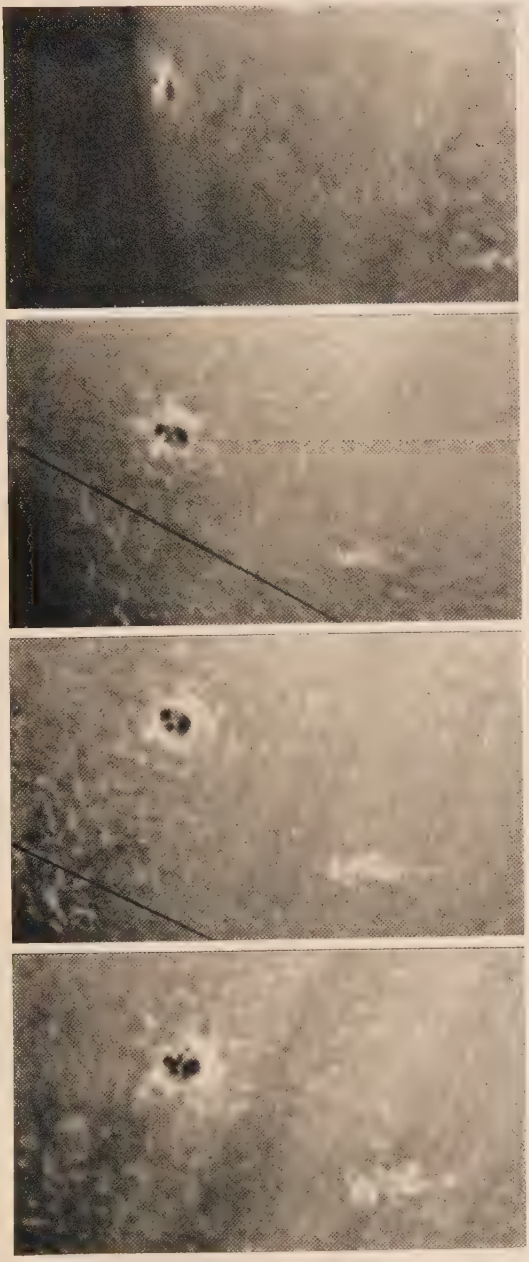
September 23.—No record.

The deduction that can be made from an examination of these photographs is that the group as a whole displays a tendency of increasing activity, and its behaviour differs in a marked way from the usual changes recorded with the spectroheliograph. In fact, even up to this stage there is something *special* to the group.

To continue and end the history of this group as recorded in calcium light we now come to September 24. On that day four photographs of the disc were secured, and no others were possible until October 5, in consequence of unfavourable weather.

September 24, $10^h\ 5^m\ 50^s$.—This photograph (Plate 2, No. 5) shows a complete transformation of the large flocculus group in comparison with those taken previously. The two most striking features are the great increase in intensity, and the nearly complete change of form. The flocculus group towards the south-west maintains its form and intensity, as exhibited on the 21st and 22nd, showing that the disturbed area was local to the other group. Special attention should be drawn to the great brilliancy of the calcium cloud in the north-western quadrant of the group, which seems to have the appearance of a portion of a spiral.

The two dark, more or less elliptical spots, darker than the general solar surface, are evidently portions of the umbræ of the



1. 1909 Sept. 18 10 h. 28 m. 36 s.

2. Sept. 20 9 h. 58 m. 30 s.

3. Sept. 21 12 h. 12 m. 10 s.

4. Sept. 22 10 h. 42 m. 2 s.

EAST. WEST.

previous days, which have not been completely obliterated from view by an overhanging calcium cloud.

The forms and positions of the outlying less bright portions in this and the following photographs should be noticed, as it will be seen that they suffer little if any change.

Turning attention now to photograph No. 6, about five minutes later ($10^h\ 10^m\ 59^s$), it will be seen that the general features in No. 5 are maintained, but the intensities in different portions are somewhat altered. Thus the cloud in the north-west quadrant is more clearly divided into two swirls, while the darker of the two umbræ has its major axis more north and south than north-west and south-east. The two patches of flocculus on the eastern side are somewhat smaller and less intense. The surrounding fainter flocculi are about the same as before, as is also the large flocculus group towards the south-west.

Coming now to photograph No. 7, taken about one hour later ($11^h\ 11^m\ 17^s$), we have here again quite a transformation. The intensity of the whole group is very considerably reduced, as comparison with the brightness of the south-western flocculus will demonstrate: while the total area of the bright flocculus is reduced, the two dark elliptical umbræ are now represented by one large umbra, with two bright portions stretching across and nearly meeting in the centre. The outlying portions of fainter flocculi still maintain their same shape and positions as before, and in like manner the south-western flocculus group has suffered no alteration.

Another photograph, No. 8, taken about five minutes afterwards ($11^h\ 16^m\ 24^s$), shows that changes are still going on, but nothing so violent as those which occurred previously. In this photograph the umbra is more distinctly divided into two portions. The outlying flocculi, including the group towards the south-west, remain still the same.

It is very fortunate that photographs previous to those secured on the 24th were available, for they demonstrate, I think conclusively, that the umbræ were only temporarily hidden from view by an intervening calcium cloud. The previous photographs further help to determine the time of the maximum intensity of activity of the solar disturbance, but it is a pity no record was obtained on September 23.

The gradual crescendo of activity up to the 22nd, and the abrupt change in appearance of the group between $10^h\ 10^m$ and $11^h\ 11^m$ on the morning of the 24th, suggest that possibly 10 o'clock on the 24th might approximately represent the time of greatest activity.

The series of photographs here illustrated shows, I think, with little doubt, that we are, in photographs Nos. 5 and 6, in figure 2, in the presence of a large active prominence, situated above the ordinary solar level which produces the usual "K_2" flocculi.

Attention has already been drawn to the fact that the outlying less intense flocculus groups, including the large group in the south-

west direction, have all remained practically constant in form during the time included in these photographs. This indicates that no abnormal disturbance was taking place generally at that level.

The solar storm is therefore quite local to the eastern spot group, and is situated above the ordinary "K_2" level. It should be mentioned, however, that these remarks only refer to the appearances as photographed with the "K_2" line. Perhaps the disturbance might have been seen more generally distributed if one of the hydrogen lines had been employed.

The photographs demonstrate, I think clearly, that very considerable activity was in operation in the eastern spot group at about $10^h\ 5^m$ a.m., while after $11^h\ 11^m$ a.m. it had distinctly subsided. So far as I am aware, no accounts of visual spectroscopic observations of this spot about the time of the above-mentioned epoch of activity have been published, but observations subsequent to this have been recorded.

Thus, Mr Edwin Holmes observed the spot spectroscopically at 11.30 a.m. on the 24th, and in his letter to the *Observatory* (No. 414, October, p. 396), he writes:—

"I made an attempt to see prominences, but getting the sun-spot on the slit I saw at once that when the spot-shade crossed the C line it was not dark but brilliant, far more brilliant than the general surface of the Sun there, for I was able to see it as a considerable-sized luminosity over both members of the spot with slit open to a large extent. The time was 11.30 a.m., and about an hour afterwards the brilliancy faded somewhat. . . ."

Again, according to a communication to *Nature* (September 30, p. 396), Professor Fowler observed the spot visually at 12.20 p.m. on the same day. He wrote: "As to the sun-spot, there was a brilliant reversal of the C line of hydrogen over one of the umbræ when I observed it at 12.20 p.m. on September 24, and on opening the slit it was clear that this appearance was produced by a very bright overlying prominence. Reversals of the chromospheric lines *b* and 1474K were also suspected, but the observations were stopped by clouds. According to Tacchini and Lockyer, it is the prominence rather than the spot which should be considered, as related to the magnetic disturbance."

Both the above extracts indicate that at the times of observation the spot group still showed signs of activity, and Professor Fowler also makes the suggestion of the existence of a prominence overlying the spot region.

Both these observers, however, were unfortunate enough to miss the time of greatest activity, but perhaps observations by others may be subsequently published.

On the following day (September 25), the day of the magnetic storm, unfavourable weather prevented any observations of the spot being made at South Kensington. Mr. A. A. Buss on that day records his observations, and he writes (*Observatory*, No. 414, October, p. 395):—

"No conspicuous increase or change was noted of the group as a whole, the concentration persisting, although showing much change of detail. Spectroscopic activity could not be ascertained, as the definition that day was not good enough for that purpose."

There seems reason to believe that the eastern spot group was, during the interval of time here dealt with, most active at about 10 o'clock on September 24. If, therefore, this spot group is to be considered as associated with the great magnetic storm of September 25, then it seems probable that the original magnetic impulse was given at the time of the greatest activity in the spot region as above stated.

Taking 4.30 p.m. on September 25 as the epoch of the maximum intensity of the magnetic storm, we can now compare this time with those of the greatest recorded activity of the spot group. Arranging them in the order of time, the following table is formed.

Greatest recorded prominence activity, Sept. 24, 10 a.m.
Magnetic storm at maximum, Sept. 25, 4.30 p.m.

From the above it will be seen that the magnetic storm has a lag of 30·5 hours on the prominence activity.

The actual total obscuration and reappearance of an umbra of a spot by the formation of an overhanging prominence has been recorded by Professor Hale * in the case of a solar eruption photographed on July 15, 1892, with the spectroheliograph of the Kenwood Observatory. This solar disturbance, which he describes as "a phenomenon which in suddenness of appearance, exceptional brilliancy, and rapidity of change in form and brightness might almost be ranked with the classic observations of Carrington and Hodgson in 1859," was at its height at $11^h\ 47^m$ (Chicago mean time), or $5^h\ 47^m$ p.m. G.M.T. It was followed by a great magnetic storm which set in and lasted over the 16th until midnight of the 17th, the maximum range of the magnets occurring at 7 p.m., G.M.T., on the 16th.

Thus between the greatest solar activity and the largest magnetic range there appeared to be a lag of about 25 hours.

The following table brings together the facts concerning these two remarkable solar disturbances, and the subsequent magnetic storms: the times of the abrupt beginning of the magnetic storms have also been inserted.

1909 (*Lockyer*).

	Sept.		Diff.	Diff.
Max. Prominence Activity	24	10 a.m.	25·75 hrs.	30·5 hrs.
Beginning of Magnetic Storm	25	11·75 a.m.		
Max. of Magnetic Storm	25	4·5 p.m.		

* *Astronomy* and *Astro-Physics*, vol. xi. p. 917, 1892.

	1892 (*Hale*).		Diff.	Diff.
	July			
Max. Prominence Activity	15	5·7 p.m.	21·3 hrs.	25 3 hrs.
Beginning of Magnetic Storm	16	3 p.m.		
Max. of Magnetic Storm	16	7 p.m.		

An examination has been made of a series of paper prints taken from photographs of the disturbed spot region obtained at Dehra Dun with the photoheliograph. The negatives, to which reference need only be made here, were obtained on the days and at the times given in the following table:—

	h	m		
September 22	8	18·5	a.m.	G.M.T.
23	2	20·1	,,	,,
24	11	24·8	,,	,,
25	10	56·4	,,	,,

In all of these the umbræ are clearly defined, and the group as a whole, *i.e.* umbræ and penumbræ, indicates a progressive increase in area, with not very great alteration in form. Unfortunately the photograph taken on the 24th was secured after those obtained with the spectroheliograph, showing the umbræ nearly hidden from view (Plate 2, Nos. 5 and 6).

In conclusion, it is of interest to note that considerable magnetic disturbance was recorded at Kew on October 18–19, though nothing at all approaching that recorded on the above occasion.

According to a recent communication to *Nature* (October 28, 1909, p. 524), "the whole afternoon of October 18 was slightly disturbed magnetically, but there was a marked sudden development about 8.30 p.m., and a considerable disturbance prevailed thereafter until 6 a.m. on October 19. . . ." Further, an unusually brilliant display of aurora on the same night—October 18–19—was reported over practically the whole of the Transatlantic trade route between Europe and the United States of America.

Thus there was an interval of 23 days 8 hours between the beginnings of these two magnetic disturbances, a period of time shorter by more than 2 days than the period of rotation of the Sun for the latitude of the disturbed solar region.

Unfortunately no record of the Sun's disc in "K_2" light could be secured on the 16th, 17th, or 18th of October, but on the 19th a photograph displays the same disturbed region. The group is of considerably greater area than before, judging by the calcium flocculi, and at the time the photograph was taken ($2^h\ 9^m\ 53^s$ G.M.T.) it was to the eastward of the central meridian. The group as a whole presents no unusual appearance.

The excellent series of photographs discussed in this paper were obtained by Messrs. T. F. Connolly, B.Sc., and W. Moss, and they were prepared for reproduction by Mr. J. P. Wilkie, photographer in the observatory.

The Sun-spots and associated Magnetic Storms of September–October 1909. By the Reverend A. L. Cortie, S.J.

1. Since the beginning of the present year until the 20th of July there has been a steady decline in the sun-spot activity of the Sun. A sufficiently accurate knowledge of the tendency of sun-spot activity can be gained from the tabulation of the mean daily number of groups observed each month, independently of the areas of the groups. Forming yearly means, the number for 1907 was 4·0, for 1908 3·4, and for the present year until the end of October 2·6. The monthly results from the Stonyhurst drawings for the period January–October of the present year are—

Month.	1909. Number of Disc Drawings.	Mean Daily Number of Groups.
January	12	4·2
February	14	3·2
March	12	3·4
April	25	2·8
May	28	2·4
June	18	1·9
July	12	1·8
August	17	2·1
September	20	1·7
October	16	3·8

A revival of activity in the last days of July was short-lived, though it was marked by the passage of a sun-spot group across the visible disc, of much greater area than the group which accompanied the very great magnetic storm of September 25, to be described subsequently. The magnets, with the exception of a moderate disturbance on July 23, showed but small movements. The month of August was likewise quiet, such groups as appeared being composed of small spots, and the faculæ, though sometimes extensive, being faint. Among these small groups was a small roundish spot 1/10 of a unit (unit = 1/5000 of the visible disc) in area which was first drawn on August 2, and was a mere dot on August 4, in position latitude − 5°, and longitude 308°·4; this small spot was the first appearance of the spot which subsequently accompanied the great magnetic storm of September 25. It had almost disappeared on August 4. There were moderate disturbances of the magnets on August 1 and 2, the greatest range of the declination magnet being 22′, and of the horizontal force 121·5, 10^{-5} C.G.S. units, the spot being 30° west of the central meridian. If this spot group developed normally, the active phases in its life-history, corresponding to Types II. and III. of the Stonyhurst system, were passed through on the invisible hemisphere during its

second transit. It is most probable that a new outburst near the position of this small spot occurred on that hemisphere.

2. On August 31 a group of small spots appeared amid moderately bright and extensive faculæ near the Sun's E. limb. This group persisted on September 2, but had disappeared by September 5. Otherwise the Sun was free from spots. Nevertheless the magnets on August 29 and 30 suffered a moderate disturbance greater than that which marked the beginning of the month. The maximum range of the declination was 32′, and of the horizontal force 157·5. This disturbance, separated by an interval of twenty-eight days from that of August 1–2, might be attributed to the influence of the region of the small spot of August 2–4, which was the precursor of that of September 25. This region was then 28° west of the central meridian. It would be very interesting if spectroheliograms of the Sun's flocculi showed a disturbance in this region, and it would be worth while for those who possess such spectroheliograms to investigate the matter.

3. The drawing of September 5 showed a group of small detached spots, which next day had coalesced into a group of Type II. A similar set of small spots formed about 10° above it in latitude, which grew rapidly, and these two groups grew simultaneously until they became two fine trains of spots between September 8 and September 14, when they disappeared on the Sun's west limb. They were the only two groups on the Sun's surface between September 6 and September 14, and were an excellent example of the sympathetic and identical development of two evidently related groups. They crossed the central meridian on September 7, and reached their full development two days later. Their passage across the Sun's disc was marked by moderate magnetic disturbances on September 3, 5, 7, 8, that of the 3rd being the greatest. As there were no other spots on the Sun, if these magnetic disturbances are to be attributed to the influence of these active groups, it follows that the same spot can influence the Earth's magnetism in more than one position on the Sun, which seems adverse to the possibility of the mode of influence being in the form of one stream of electrified particles. Many instances of spots being thus magnetically active on more than one meridian during their transit across the Sun's disc have been observed, and this objection to an electrified stream of particles, emanating from a spot and revolving with it, being the mode of operation of a solar disturbance, was particularly insisted upon in a former paper (*Monthly Notices*, vol. lxv. pp. 197–205). Nor is there any special virtue in a central meridian position of a spot for the exercise of its influence. One of the results of the elaborate study of the magnetic storms recorded at Stonyhurst, and the associated sun-spots, during the eighteen years 1881–1898 inclusive, set forth by Father Sidgreaves in his paper "On the connection between Solar Spots and Earth's Magnetic Storms" (*Memoirs*, vol. liv. pp. 85–96), was that the western limb position of a spot was more efficacious than either the central meridian or east-limb positions, the percentages being 25, 19, and

18 respectively. The positions of the leading spots of the two groups under discussion on September 8 were, latitude 1°·8, longitude 159°·3, and latitude − 11°·8, longitude 158°·3, respectively.

4. On September 15 the Sun's visible disc was free from all spots. On September 16 two small dots in faculæ on the E. limb were observed, which were still visible on September 17. On this day the spot associated with the great magnetic storm entered on the E. limb, and, with the exception of a very small group of spots in higher latitude, was the only spot on the disc. On the 18th the group consisted of two distinct spots, which were united into one on the 19th, and persisted as two small nuclei in a dense cloud of penumbral matter, until its disappearance at the W. limb on September 29. The spot, however, though large, was never a really big one, its maximum area being about 6 units. On the 21st there was a magnetic storm, which deserves to be reckoned as great, the maximum range of declination being 38′, and of horizontal force 126·0, 10^{-5} C.G.S. units. Was this storm due to the presence of the spot on the Sun? It forms no sequence of 27 days with anything gone before it, though it does with one that comes after it. It is half a period, too, after the disturbance connected with the spot groups of September 7–8. It was followed on the 25th by one of the biggest storms recorded since 1882. The details of this great storm have been given by both Dr. Chree and by Father Sidgreaves in *Nature*, Nos. 2083, 2084. Suffice it to say, that its character was of very large and rapid oscillations, the light spots of the three instruments being continually beyond the limits of the recording papers, especially between 3.30 p.m. and 5.30 p.m., while just before the end of the storm at 8.30 p.m. the declination needle swung right across the paper, or more than 2°·75, in less than two minutes of time, accompanied by an equally violent oscillation of the horizontal force magnet, the vertical force needle being pulled off its balance. Brilliant auroræ were simultaneously observed at several stations in both north and south latitudes, noteworthy being the observation of the chief auroral line by Professor Fowler in a clouded sky.

Spectroscopically, the spot was an active one, as recorded by several observers, among them Mr. Buss, whose series of observations has appeared in the *Observatory*, No. 414, and Professor Fowler. It was still active on September 29, when near the W. limb, according to my own observations. Yet it was nothing very much out of the common, for Mr. Buss remarks (*loc. cit.*): "Nevertheless, I feel somewhat astonished at what has taken place, as I have seen so many much more active and excited disturbances crossing the Sun's disc without leaving such an extraordinary mark," an opinion in which Professor Riccò concurs with regard to the daily spectroheliograms of the spot region made at Catania (*Nature*, No. 2088). There is, however, one observation that must not be passed over in silence. On M. Deslandres' photograph taken in the high level H_α light on September 24 is a long black filament in the S. hemisphere, of which there is only a faint

tracing left on September 25. Similar filaments have sometimes endured for several solar rotations. The position of the spot on September 23 was latitude $-3^{\circ}\cdot5$, longitude $305^{\circ}\cdot6$, on which day it crossed the central meridian. The great magnetic storm occurred when it was about 20° west of the central meridian. It is difficult to understand how a moderate-sized spot, when compared with very many others, and not exceptionally active spectroscopically, at a time, too, of comparative minimum of solar activity, could have directly caused so great a storm.

5. As this spot was going off the Sun's west limb on September 29, another spot of very similar character, and subsequently spectroscopically very active according to Mr. Buss's observations, though quiet on 29th when observed by myself, had just come in on the east limb, right on the solar equator, and in longitude $166^{\circ}\cdot4$. There was a noteworthy magnetic disturbance on the 30th, with a range of declination 38′, and of horizontal force 315·0, 10^{-5} C.G.S. units. This spot was quite as large as its predecessor, and at the time of the magnetic disturbance was about 42° east of the central meridian.

6. Quite a revival of sun-spot activity took place in the early days of October. The record of magnetic disturbances during the month contains two great storms; the first, the finer one, on October 18, with a range in declination of 55′, and in horizontal force of 216·0, and the second on October 23, with ranges in the two elements of 36′, and 162·0, 10^{-5} C.G.S. units. These two storms follow those of September 21 and September 25 at an interval of twenty-seven days, both the September storms having been associated with the spot of September 18–29. The spot had returned, but now as a long group in extensive faculæ, and it increased in area between October 16 and October 18. The heliographic co-ordinates of the leader spot of the group were latitude $-4^{\circ}\cdot6$, longitude $312^{\circ}\cdot3$, and of the following spot latitude $-7^{\circ}\cdot7$, longitude $306^{\circ}\cdot3$.

7. The results of this analysis of the magnetic disturbances discussed may be exhibited in tabular form as follows:—

No.	Date.	Range.		Character.
		Declination Minutes of Arc.	H. Force 10^{-5} C.G.S. Units.	
1	Aug. 2	22	121·5	Moderate.
2	,, 30	32	157·5	,,
3	Sept. 3	28	85·5	,,
4	,, 5	22	72·0	,,
5	,, 7	18	85·5	,,
6	,, 8	17	67·5	,,
7	,, 21	38	126·0	Great.
8	,, 25	>165	>742·0	Very great.
9	,, 30	38	315·0	Great.
10	Oct. 18	55	216·0	,,
11	,, 23	36	162·0	,,

The disturbances numbered 1, 2, 8, 11, form a twenty-seven day sequence connected with the spot of September 18–29. Disturbances 7 and 10 form another twenty-seven day sequence also connected with the same spot, unless the evidence of spectroheliograms should show a disturbance independent of the spot region. The moderate disturbances 3, 4, 5, 6 were connected with the birth and rapid development of the two spot groups of September 5–14. It follows that a position of advantage of a spot group for influencing the magnetic state of the Earth may occur on more than one meridian during the same transit of the disc. Nor is there any special importance in a central meridian position. Finally, there is no relation in magnitude between the storm of September 25 and the accompanying solar disturbances.

Stonyhurst College Observatory:
1909 *November* 8.

A Solar Outburst and a Magnetic Storm.

By C. Michie Smith, Director of the Kodaikánal Observatory.

Cases of undoubted connection between solar outbursts and terrestrial phenomena are sufficiently rare to make it worth while placing on record full details of such a phenomenon.

On the forenoon of September 28, the First Assistant, Mr. Sitarama Aiyar, while examining the spectrum in the region surrounding the large spot, which was then approaching the west limb (approximate position lat. 5° S., long. 307°), observed what he (probably rightly) described as a large prominence overlying the spot. This was at $10^h\ 30^m$ I.S.T. The spot had already been examined at $8^h\ 23^m$, when C was found to be reversed on the spot, and a prominence had been observed on the limb at 6° south at $10^h\ 0^m$. Neither of these features was, however, in any way striking. The eruption seen at $10^h\ 30^m$, on the other hand, was of a very striking character, for C was so brilliantly reversed that, in spite of a poor sky, the shape of the eruption could be clearly traced with a comparatively wide slit, and it was seen to be changing rapidly.

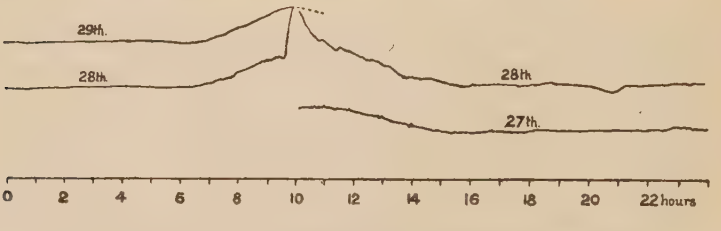
At $11^h\ 10^m$ the F line was displaced 0·4 Å to red and 1 Å to violet; at $11^h\ 3^m$ the displacement was 1·7 Å to violet. By 11^h the eruption seemed to be less massive than when first seen, but it was still bright, and D_1, D_2, D_3, b_1, b_2, b_3, b_4 were all brightly reversed on one of the umbræ. D_3 was very dark over the whole prominence on the limb side of the spot at $11^h\ 23^m$.

Spectroheliograms taken at $7^h\ 55^m$ and $8^h\ 20^m$ showed no peculiarities, and the umbræ of the spot were clear and sharply defined. Another plate, taken at $10^h\ 39^m$, was quite different. The definition had, as usual, fallen off considerably, but it was good

enough to show quite clearly that a great eruption had taken place, covering practically the whole spot region. The umbræ could now be only faintly traced, and there were, instead, several small regions of intense brilliancy. Clouds prevented farther observations on that day. Next day was also cloudy, but on the 30th. a number of spectroheliograms were obtained, which showed an eruptive prominence of a rather remarkable form issuing from near the spot.

On the development of the magnetograph sheets it was found that a very sudden rise had taken place in the horizontal force at or near the time of the eruption. The rise began at $10^h\ 3^m$ and amounted to 154γ in 20^m, while the curve did not return to its normal position till between 14^h and 15^h. The vertical force and declination curves showed small "kicks" at the same time.

It is impossible to say whether or not the solar outburst and the magnetic disturbance were exactly simultaneous, as the sky near the Sun was covered with clouds between $10^h\ 3^m$ and $10^h\ 30^m$, but had the outburst taken place before 10^h it could hardly have been



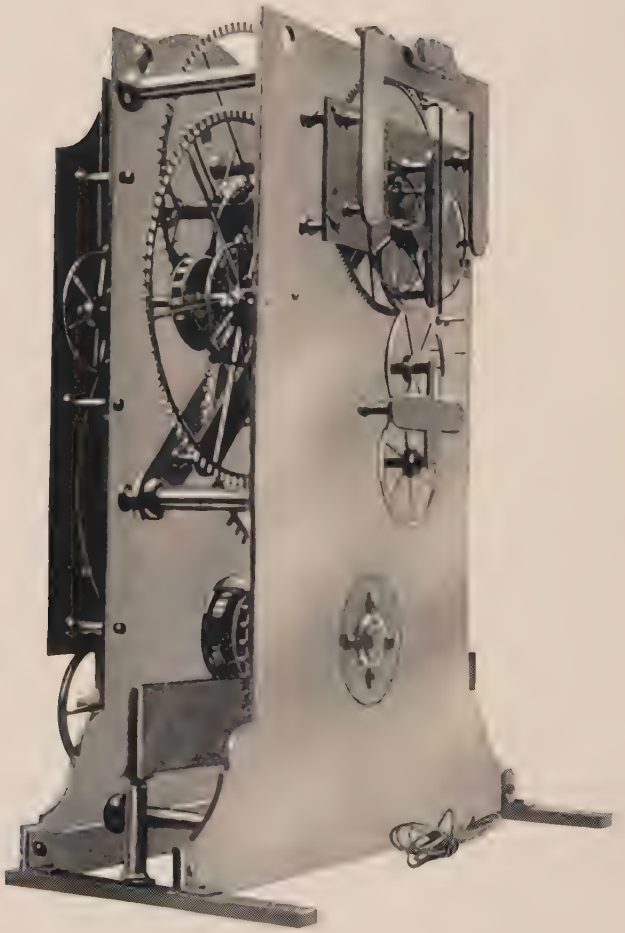
Horizontal Force, 1909 September 27–29, Kodaikánal Observatory.

overlooked when the prominence on the limb near it was being examined. In confirmation of this it may be mentioned that a photograph of the spectrum of the spot taken at $10^h\ 1^m$ showed nothing unusual, while one taken at $11^h\ 9^m$ showed very strong reversals in H, K, and H_ϵ.

The disturbance was, of course, small compared with what was recorded in the great storm of the 25th September, when the change in the H.F. was about five times as great. Unfortunately that disturbance began in the afternoon, when the Sun was not under examination, and we have no records to show whether or not it was accompanied by a great solar outburst.

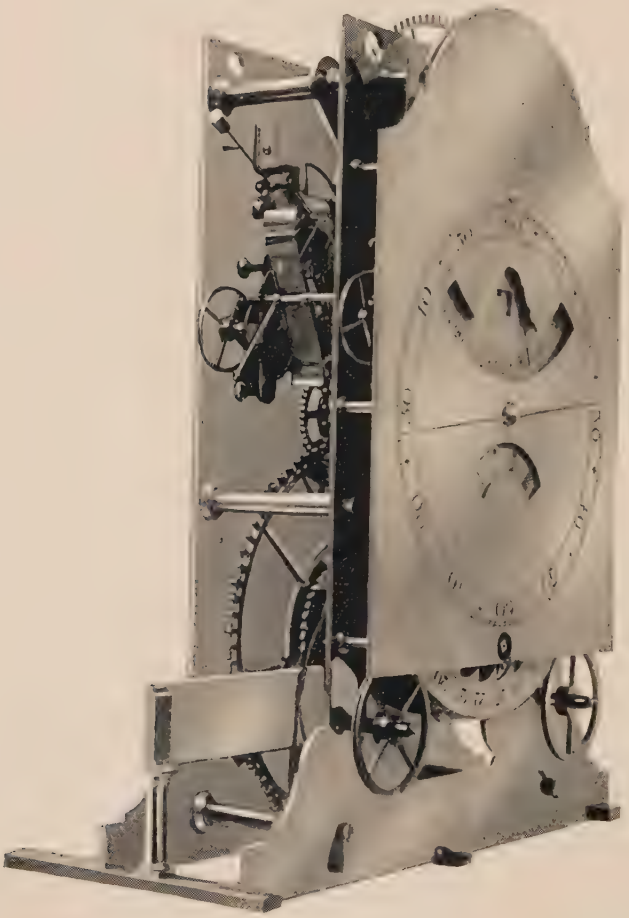
Without venturing to express any opinion as to the nature of the connection between terrestrial magnetic storms and solar conditions, it may be well to point out that, so far as my experience goes, a spot which is spectroscopically active is much more likely to be accompanied by magnetic disturbances than one which is quiet.

Kodaikánal Observatory:
1909 *October* 7.



HARRISON CLOCK.

View of escapement, and uncovered roller bearing to main wheel, with friction wheel bearings to the centre and escape wheels.



Harrison Clock.

View of dial, showing double minute hand, hour and day of month circles, and the fly (or fan) of remontoire.

A Description of the Society's Harrison Clock, with a brief account of the Maker. By E. T. Cottingham. (Plates 3, 4).

John Harrison, the son of a carpenter, was born at Foulby, in Yorkshire, in 1693, fifty-five years after Tompion, "the father of English clockmaking," and twenty years after Graham, the inventor of the dead-beat escapement, so that, in order of birth, Harrison was the third noted maker of timekeepers.

It is recorded that he followed his father's business in early life, and afterwards acquired the art of clockmaking without tuition.

The construction of this clock seems to indicate that he had an unusual knowledge of wood, by the manner he has brought lignum-vitae into use in the construction of the wood rollers of the lantern pinions, and also for the pallets of this most ingenious escapement.

On one occasion he was sent for to attend a clock, and on arriving found that the pallets had become dry, and only needed oiling. He was somewhat blamed for the dry pallets, and was rather annoyed at the treatment he received after having travelled so far to correct it, and on returning invented this escapement (which goes without oil), so that no other person should meet with such treatment from the same cause.

The chief points of interest are:—

The silent escapement, with its lignum-vitae pallets.
The remontoire.
The roller-bearing to the main wheel.
The friction wheels to the other bearings.
The high quality of the work generally.

The escapement has a large but very light escape-wheel of 120 teeth, which turns once in four minutes, one tooth of which escapes with every two beats of the seconds pendulum.

Eight fine pins placed round the centre of the wheel at about ·25 of an inch radius engage in turn with a delicate brass-forked lever, which releases the other wheels and allows the minute-hand to move forward at half-minute intervals.

The axis of the crutch (which imparts motion to the pendulum) usually carries the pallets, and the two have a common centre of motion. In this clock the crutch carries a rectangular frame, marked A in fig. 1; and in this frame the two wood pallets and two small brass gravity-arms work on a common axis at B, ·9 of an inch from the centre A.

The pallet axis B therefore has a circular motion equal to that of the pendulum.

The crutch D has just completed its swing to the right, and the pallet E engages the tooth F silently and without friction, causing the wheel to recoil slightly.

As soon as the wheel recoils, the pallet G, being slightly

weighted at its opposite end L, rises clear of the tooth H until it

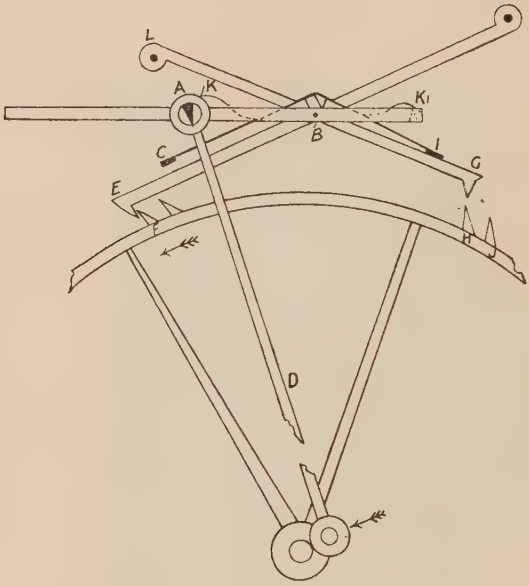


FIG. 1.—Drawing of the Escapement.

FIG. 2.—Photograph showing the Pallets mounted in the frame A, and knife-edges.

meets the heavier brass arm I, which is momentarily lifted from its stop K_1 by the slight impact.

The tooth F now forces the pendulum to the left until the pallet G meets the tooth J, when it acts as a toggle joint in giving a slight recoil to the wheel again, and releases the pallet E until checked by contact with the heavier arm C. The tooth J, engaging with pallet G, forces the pendulum to the right again, to complete the escaping cycle of one tooth. The pallet E enters the wheel because it is in contact with C, but in the drawing they are separated as they will be when the crutch D has nearly completed its swing to the left.

The crutch axis works on knife-edges resting in agate V's.

In other clocks the release of the escape-wheel teeth from one pallet to the other causes the tick; but in this it will be seen there is no such impact, as the pallets meet the teeth with a very slow motion, and the beat of the clock is practically inaudible, so that, from an astronomer's point of view, it needs a seconds contact and a sounder.

There is no rubbing friction on the pallets, and that disturbing factor, oil, is not required.

Although there is a very slight recoil of the escape-wheel, it would be a great injustice to Harrison to compare it with that commonly known as the recoil escapement, for the working conditions are so different; but it is much more delicate and needs great care in adjustment, which is doubtless the reason why it died with the inventor.

The function of the remontoire is to maintain a practically uniform pressure at the escape-wheel teeth. The escape-wheel pinion works through a hollow spindle or pinion, and the two are connected by a watch-spring, which means they must make the same number of revolutions per hour. Every half-minute the escape-wheel unlocks the remontoire, and allows the wheel on the hollow pinion to move forward half a minute, and in doing so winds up the watch-spring from the outer end.

The inner end of the watch-spring unwinds an equal amount by turning the escape-wheel one half-minute, when another rewinding takes place. Thus the action of the remontoire cuts off the variable gearing or train friction from the escapement.

It must not be lost sight of that the latter has to release the remontoire, which, if not well designed, creates much friction.

All the arbors (axles) in the clock are hard gun-metal, and pass through enlarged holes in the plates, to rest on pairs of light friction-wheels with lignum-vitae bushes, working on fine brass pins on the outside of the plates. The brass pins on which the bushes turn show no signs of corrosion, and were quite bright when removed, although the clock has been standing for many years under the unfavourable conditions of a London atmosphere.

The main wheel works in beautifully finished roller-bearings, the rollers of which are made from a very hard non-rusting alloy, and are kept from touching each other by working on fine brass pins in a revolving brass carriage. This represents in design the

latest practice in engineering of to-day, although made a hundred and seventy years ago.

Between this clock and the simply constructed dead-beat escapement clocks of Graham—which gave a much closer rate without a remontoire—there is as much difference as between Harrison's beautifully executed but complex timekeeper—(with which he took the first government grant of £20,000 in 1764) *—and the simple spring detent chronometer invented soon afterwards.

It is interesting to trace the evolution of ideas and early training from Harrison's first efforts with timekeepers, made with wheels entirely of wood (with the exception of the escape-wheel), one of which was going in the seventies in the Patent Museum at South Kensington—to that which culminated in the £20,000 award, in which the use of wood disappears.

Although this form of clock has not survived in practice, the maintaining power, and this form of roller-bearing, and the 18,000 train (a mechanism beating fifths of a second, or 18,000 per hour) have proved to be lasting conceptions of this ingenious man.

Observations of Jupiter's Galilean Satellites, 1909 *Jan.–Aug.* By R. T. A. Innes, Director of the Transvaal Observatory. (Plate 5.)

These follow on the lines of the observations in 1908 (*Monthly Notices*, 1909 April). They were made with the Grubb 9-inch refractor with powers of $\frac{1}{5}$th or $\frac{1}{6}$th of an inch on a focal length of 130 inches. As a rule, each phenomenon depends on 5 phases, viz.—

First contact,
$\frac{1}{4}$ of disk on or off,
Middle phase,
$\frac{3}{4}$ of disk on or off,
Last contact,

but instead of taking the simple mean, as in 1908, the weights 3, 1, 2, 1, 3, respectively, have generally been used. The mean time so deduced is alone given. Everything that experience could suggest was adopted to ensure accuracy. The moments of disappearance or reappearance in eclipses and the first contacts of the other phenomena were taken by the sounder, but other phases were taken by the chronometer, so as to avoid bias.

The Cape scale of definition is adopted, viz. 1 for perfect seeing, and 5 for very bad seeing. As at the Cape, it was noticed that the best definition occurs with winds from the equator, and the worst with winds from the pole, and the lighter the wind the

* He only received £5000 in 1764, the remainder of the award not being paid him till 1773.

better the seeing. It would be interesting to know if this is a general experience. (*N.B.*—Using the words "wind from the equator," and so on, instead of N. wind, saves specifying the particular hemisphere referred to. Roughly, the barometer falls with a wind from the equator, and rises with a wind from a pole.)

I hope to continue this series under identical circumstances until Jupiter has made one revolution. I am encouraged to undertake this duty because I feel that the observations are of considerable accuracy; but in addition to my own sentiments, I am giving effect to a very strongly expressed wish of Laplace's in 1805. He wrote as follows—(*Mec. Cel.*, iv., preface, p. xv):—

"The observations of the ingress and egress of the satellites and of their shadows on the disk of Jupiter will much extend our knowledge of the size of their disks, and of several other elements of the theory of the satellites. It seems to me that this sort of observations, too much neglected by astronomers, ought to fix their attention; because it is evident that the interior contacts of the shadows should determine the instant of conjunction with still more exactitude than the eclipses. The theory of the satellites is now sufficiently advanced, that what is lacking can only be determined by very precise observations; it has therefore become necessary to try new means of observations, or at least to assure oneself that those of which use is made merit the preference."

Again (chap. xvi. p. 172):—

"The observations of the eclipses of Jupiter by its satellites can, in general, much expand the knowledge of their theories; nearly always both commencement and end can be observed, and this remark applies both to the satellites and their shadows; it is really equivalent to four observations, whilst most often only the commencement or the end of the eclipses of the satellites can be observed. This method of observation, too much neglected by astronomers, appears to me to merit all their attention."

It is true that contact phenomena have been widely observed in the past, but the very low magnifying powers invariably used have prevented the attainment of any great accuracy.

Were there a good theory of the phenomena of the satellites extant, I would have instituted a comparison of the observations with it. By the time that this is in print, we may expect to have the results of Professor Sampson's great work on the Harvard observations in our hands; and if his tables readily apply themselves to the contact phenomena, the comparison will be made. In its present state, Professor de Sitter's theory founded on heliometric and photographic observations is not well adapted for computation of the contact phenomena.

1909.	Phenomenon.	No. of Phases.	Central Phase Greenwich Time. h	m	s	p.e.	Deduced Semi-duration. s	Estimated Latitude. °	Remarks.	Definition.
Jan. 30	II. E D	3	8	17	29·0	± 3·5	100·0	...	By disk	
	Last speck		8	19	27·7					
	IV. E D	3	12	0	9·4	±10·0	230·0	...	By disk, oblique shape of shadow seen	
	Last speck		12	8	59·0				Good, but very slow	
	II. O R	5	12	26	56·7	± 5·7	109·0	− 10		2
Feb. 1	III. E D	4	10	54	27·6	± 9·0	214·0		By disk, phases well seen, oblique about 45°	
	Last speck		10	58	35·6				Glass wet with dew	
5	I. S I	5	9	7	4·9	± 3·0	82·4	+10		3
	I. T I	5	9	37	38·0	± 2·0	91·3	+12		
	I. S E	5	11	22	26·4	± 3·2	90·4			
	I. T E	5	11	53	23·6	± 2·2	95·4			3
13	I. E D	3	8	9	35·2	± 0·9	74·0		By disk, phases well seen	
	Last speck		8	11	7·3					
14	I. S E	3	7	44	29·5	± 2·0			Clouds, poor	5
24	II. O R	4	8	22	41·5	± 4·7	97·3	− 8		
Mar. 1	I. O D	3	6	24	30·3	± 4·5	[81]		Bad, is perhaps 22 secs. too late	5
	I. First speck		8	40	23·3				Very close to Jupiter	
	I. E R	4	8	41	36·7	± 2·0	80·6	− 5	By disk	3
9	I. T I	5	5	27	18·8	± 3·5	[80·0]	+ 8	Poor	5
13	IV. S I	5	7	35	59·8	± 7·1	280·0	+40	IV. visible on disk as a large dark spot. Shadow darker and larger than IV.	3
15	I. O D	5	9	51	8·7	± 3·2	87·0	− 5	Poor, passing mist	3

16	I. T I	5	7	10	18·3	± 4·5	110·2	+ 5	Exactly on thin dark belt	3
	I. S I	5	7	34	12·0	± 3·0	90·0	+ 5		4
	III. O D	5	9	10	47·4	±11·8	237·2	−25	First and last contacts double weight	3
	I. T E	5	9	26	34·5	± 3·0	105·4	+ 5	,,	3
	I. S E	5	9	49	10·7	± 3·0	104·4	+ 5		3
17	I. First speck		6	56	46·2					3
	I. E R	4	6	58	25·8	± 1·9	90·0	− 4	By disks, but poor	3
23	I. T I	5	8	55	21·2	± 3·0	92·6	+ 5		3
	I. S I	5	9	28	17·9	± 2·1	89·0	+ 5		2
24	I. First speck		8	51	11·0				Through very thin cloud	
	I. E R	4	8	52	33·2	± 3·2	90·0		By disks, poor	3
25	I. T E	5	5	37	52·4	± 4·4	111·5	+ 5		3
	I. S E	5	6	11	47·7	± 4·4	109·0	+ 5		2
26	II. T I	5	8	44	14·3	± 3·2	95·7	+10		3
27	III. S I	4	4	47	16·8	± 2·0	228·0	+30 }		3
	III. T E	4	5	35	8·4	± 1·1	211·0	+30 }	Shadow is as large or larger than III.	3
	III. S E	2	8	5	41·0		240·0		Shadow obl. N. and S., contacts only retained	2
28	II. E E	First speck	7	52	1·9					3
	,,	4	7	54	33·4	± 4·8	150·0		By disk, but poor	4
30	IV. S E	2	5	38	41·7		(365)	+40	See note	2
31	I. O I	5	7	47	59·4	± 1·1	96·0	− 5		2
April 1	I. T I	5	5	8	4·6	± 6·2	(90)	+ 5	Very poor	5
	I. S I	5	5	51	48·4	± 5·0	,,	,,	Poor	4
	I. T E	5	7	23	21·1	± 2·4	114·0	,,	Very unsteady	3

1909.	Phenomenon.	No. of Phases.	Central Phase Greenwich Time.				Deduced Semi-duration.	Estimated Latitude.	Remarks.	Definition.
			h	m	s	p.e.	s	°		
April 2	I. First speck		5	14	17·3					2
	I. E R	4	5	15	57·2	± 2·0	84·0		By disk, fair	2
3	III. T I	5	5	40	25·2	± 3·5	267·5	+30		2
	III. S I	7	8	46	18·1	± 4·9	249·0			2
	III. T E	5	9	0	47·8	± 8·7	243·5			1
	III. S E	2	12	2	51·5	±25·0	230·0		First and last contacts retained	3
8	I. T I	5	6	55	22·0	± 2·0	92·0			3
	I. S I	5	7	46	36·9	± 2·4	96·0			2
	I. T E	5	9	10	50·6	± 3·9	108·0	+ 4		2
	I. S E	5	10	0	40·0	± 2·7	90·0	+ 5		3
9	I. First speck		7	8	55·4					4
	I. E R	4	7	10	29·2	± 3·5	(90)		By disk, poor	4
10	I. S E	5	4	28	45·3	± 2·8	92·0	+ 5	Twilight, poor	5
11	II. O D	5	8	28	4·3	± 3·0	97·0	− 6	Very good, power 820	3
13	II. S I	4	4	27	12·3	± 2·4	(97)		Twilight, very poor	4
	II. T E	5	5	20	40·9	± 1·8	75·0	+10		3
	II. S E	5	7	9	48·8	± 1·6	79·6		Power 820, difficult but good	3
14	III. First speck		5	55	14·4					3
	III. E R	5	5	59	47·2	± 4·4	227·5		By disk, inclination of shadow of Jupiter well seen, ±30°	3
15	I. T I	5	8	43	7·9	± 2·9	104·0	+ 5		3
	I. S I	5	9	41	53·4	± 3·5	83·5			4

	IV. T I	9	10	26	31·3	±13·1	324·0	+20	See note	2–4
16	I. O D	5	5	49	43·8	± 1·2	90·0	− 5	Power 820	2
	I. First speck		9	3	34·8					3
	I. E R	4	9	5	34·1	± 2·0	96·0		Disks, fair	3
17	I. S I	4	4	10	51·2	± 3·6	[90]	+ 5	Daylight, very poor	5
	I. S E	5	6	22	51·4	± 4·1	106·0	+ 5		3–4
22	II. E R	First speck	4	56	23·6					3
	,,	4	4	58	52·2	± 1·0	126·0		By disk, poor	3
23	I. O D	5	7	38	34·0	± 3·3	92·8	− 3	Power 820	2
24	I. T I	5	5	0	18·1	± 3·2	88·8	+ 4	Clouds at first	1
	I. S I	5	6	4	59·2	± 4·3	99·0	+ 5		2
	IV. E D	6	6	6	53·7	± 5·5	222·0		By disks, good	2
	,,	Last speck	6	17	0·4					2
	I. T E	5	7	15	17·6	± 6·8	103·6			2
	I. S E	5	8	18	9·6	± 4·1	90·5		Power 820	2–3
	IV. E R	First speck	10	3	3·2					3
	,,	7	10	11	13·9	±15·7	280·0		Poor, J. low	4
25	I. E R	First speck	5	27	20·8					2
	,,	4	5	28	49·2	± 0·9	84·0		By disks	2
27	II. T I	5	7	23	16·9	± 5·2	119·0	+10		2
	II. S I	6	9	38	17·3	± 7·2	104·0	+12	Jupiter's equat. belts, thin and dark	3
29	II. E R	First speck	7	31	39·6					3
	,,	4	7	34	4·7	± 1·8	113·2		By disks	3
30	I. O D	5	9	29	10·7	± 6·2	78·0	− 3	Poor	4

1909.	Phenomenon.	No. of Phases.	Central Phase Greenwich Time. h m s p.e.	Deduced Semi-duration. s	Estimated Latitude. °	Remarks	Definition.
May 1	I. T I	5	6 50 24·3 ± 2·1	110·0	+ 4		3
	I. S I	5	8 0 18·0 ± 4·5	90·4		Poor	4
2	III. S E	4	3 55 7·1 ± 4·3	230·0	+30	Daylight	3-4
	I. O D	5	3 57 8·7 ± 1·6	75·0	− 3	,,	4
	IV. T E	6	6 50 12·5 ± 7·1	216·5	+40	See note	3
	I. E R	First speck	7 22 14·6				2-3
	,,	4	7 23 39·5 ± 0·6	78·0		By disks	2-3
3	I. S E	5	4 41 9·4 ± 5·0	100·0	+ 5	Power 820	2
5	III. O D	6	9 44 10·0 ± 4·6	251·0	− 15	III. dimmer than limb of J. See note	3
6	II. O D	5	4 58 12·0 ± 2·3	83·0	− 10	Power 820	2
8	I. T I	5	8 42 29·7 ± 4·4	93·2			3
9	III. S I	5	4 43 30·0 ± 6·6	225·7	+30	Shadow in mid-transit as large as III.	3
	III. S E	6	7 52 39·4 ± 7·3	195·0	+22		4
	I. O D	5	5 48 41·7 ± 3·9	89·8	− 5		3
	I. E R	First speck	9 17 26·3			Too poor for disk	4
10	I. S I	5	4 23 25·7 ± 3·0	74·0			3
	I. T E	5	5 25 44·2 ± 0·5	90·0	+ 4		2
	I. S E	5	6 35 28·1 ± 6·8	108·0	+ 5		2-3
13	II. O D	5	7 28 32·5 ± 1·5	96·0	− 7		2
15	II. S I	5	4 10 18·1 ± 1·9	108·0	+ 7	Twilight	2
	II. T E	3	4 26 30·0 ± 5·0	[96]		First phases missed through lateness of predicted times	1-2

	II. S E	5	6	52	26·0	± 8·0	110·0	+15	Shadow at limb very minute	1–2
16	III. T I	5	3	36	36·1	±10·3	197·8	+20	Daylight	3–4
	III. T E	5	6	58	59·5	± 3·2	228·0	+20		2–4
	III. S I	5	8	42	47·2	± 7·5	232·0			4
	I. O D	5	7	41	35·0	± 1·6	96·7	− 5	Good	3
18	I. E R	First speck	5	41	13·3					4
	,,	4	5	43	0·0	± 1·5	97·3		Poor	4
19	IV. S I	9	7	40	47·9	± 8·3	350·0	+40		4
22	II. T I	5	4	12	58·6	± 2·2	99·0	+12		3
	II. S I	4	6	46	59·0	± 0·8	93·5	+10		3
	II. T E	5	6	59	33·7	± 1·5	104·0	+ 9		3
23	III. T I	7	7	31	5·0	±10·1	220·0	+15		4
24	II. E R	First speck	4	34	46·7				Close to I. and fair. A sudden brightening took place at $4^h\ 36^m\ 40^s$, or a little time after ¼ of the disk was disclosed.	4
	,,	4	4	37	11·3	± 1·7	114·0			4
	I. T I	5	6	57	54·5	± 2·4	95·0	+ 4		3
	I. S I	5	8	13	33·8	± 3·8	85·0	+ 5		4
25	I. O D	5	4	4	31·0	± 0·8	80·0	− 5	Good, twilight	2–3
	I. E R	First speck	7	36	17·4					3
	,,	4	7	37	48·7	± 1·0	76·0			3
26	I. T E	5	3	41	20·8	± 0·6	90·0	+ 5	Daylight	
	I. S E	5	4	53	44·2	± 1·9	102·4	+ 6		2
27	III. E R	First speck	5	48	25·2					2
	,,	4	5	52	16·3	± 4·3	213·3			3
	IV. O D	8	5	49	9·2	± 4·9	335·0	−25	IV. dim	2–3

1909.	Phenomenon.	No. of Phases.	Central Phase Greenwich Time. h m s	p.e.	Deduced Semi-duration. s	Estimated Latitude. °	Remarks.	Definition.
May 31	II. E R	First speck	7 9 39·1					3
	,,	4	7 11 53·1	± 1·2	86·7			3
June 1	I. O D	5	5 59 37·9	± 2·3	90·0	− 4	Power 820	1
2	I. T I	5	3 21 34·3	± 2·8	91·0	+ 5	Sunlight	3
	I. S I	5	4 36 18·4	± 1·8	91·1	+ 5		3
	I. T E	5	5 36 2·3	± 3·3	107·3			2–3
	I. S E	5	6 48 31·0	± 1·6	93·3	+ 6		2–3
3	I. E R	First speck	4 0 24·9				Fading twilight	2
	,,	4	4 1 42·9	± 3·0				2
	III. O R	9	4 50 52·6	± 5·9	260·8	- 15	Shading on limb of J = 0″·2 ±	3
	III. E D	7	6 36 9·9	± 3·7	279·0			3
	,,	Last speck	6 42 47·3					3
5	IV. S E	5	5 19 56·6	±12·4	312·0	+40		3
8	I. O D	5	7 55 24·6	± 1·6	[90]	− 5	Low, poor	4
9	I. T I	5	5 16 36·8	± 8·9	[90]	+ 4	Poor	4
10	III. O D	7	5 27 47·2	± 3·2	186·0	−15		4
	I. E R	First speck	5 55 36·2					4
	,,	4	5 57 12·9	± 7·1	[90]		Poor	4
13	IV. O R	7	4 39 0·7	± 8·3	331·0	−30	Shading on limb of J = 0″·2 ±	2
14	III. S E	5	3 47 0·2	± 5·9	173·8	+30	Twilight	2
	II. O D	5	7 7 51·6	± 0·8	108·7	−15		4

16	II.	S	I	5	3	57	2·1 ± 4·5	91·7	+15	Passing clouds, twilight	2
	II.	S	E	5	6	39	16·0		+15	Very poor—shadow very minute	4
	I.	T	I	5	7	13	33·6 ± 4·1	116·3		Poor	4
17	I.	O	D	5	4	21	59·0 ± 0·9	100·0	− 4		3
18	I.	T	E	5	3	57	55·6 ± 2·1	90·7	+ 4	Twilight	3
	I.	S	E	5	5	6	36·2 ± 1·9	95·0	+ 5		2
21	III.	S	I	5	4	40	24·6 ± 5·6	257·0	+30	Poor	4–5
23	II.	T	I	5	4	9	50·4 ± 2·9	97·3	+15	Twilight	3
	II.	S	I	4	6	34	36·6 ± 4·0	113·5	+18	Poor	4
	II.	T	E	5	6	57	42·8 ± 2·5		+15	Very poor	4–5
24	I.	O	D	5	6	20	15·3 ± 2·6	96·0	− 6		2
25	I.	T	I	5	3	40	39·2 ± 1·6	86·0	+ 4	Twilight	2
	II.	E	R	First speck	4	10	46·2			Faint twilight	2·3
		,,		4	4	12	37·6 ± 2·7	94·4			2–3
	I.	S	I	5	4	49	33·0 ± 1·5	99·7			1–2
	I.	T	E	5	5	56	4·6 ± 2·7	94·3			4
26	I.	E	R	First speck	4	14	45·8			Faint twilight	2
		,,		4	4	16	16·0 ± 3·1	[90·0]		Fair	2
28	III.	T	I	5	3	58	40·4 ± 3·7	214·3			2
30	IV.	E	D	3	6	21½				Very poor, definition was so bad that a power of 500 replaced the usual 630	5
		,,		Last speck	6	30	9·8				5
July 2	I.	T	I	5	5	38	47·9 ± 2·3	94·0	+ 4		3
	I.	S	I	4	6	45	22·6 ± 3·0	85·0		Poor	4
	II.	E	R	First speck	6	45	25·7				4

1909.	Phenomenon.	No. of Phases.	Central Phase Greenwich Time. h m s	p.e.	Deduced Semi-duration. s	Estimated Latitude. °	Remarks.	Definition.
July 3	I. O D	5	2 48 49·2	± 1·2	90·0		Sun shining on instrument. Fair	1–2
	I. E R	First speck	6 10 2·7					4
	,,	4	6 11 49·9	± 1·2	[90·0]		Poor	4
9	III. E R	First speck	5 42 8·6					4
	,,	3	5 45 41·0				Poor	4
10	I. O D	4	4 48 22·8	± 2·2	87·0	− 5	Poor	3
11	I. T E	5	4 22 0·2	± 2·0	75·3	+ 5	Faint twilight	2
	I. S E	5	5 19 8·7	± 1·2	109·0		Power 400	3–4
16	III. O R	5	5 59 32·7	± 4·5	168·5	− 15	Low	3
18	II. S I	4	3 45 37·7	± 3·7	93·8	+ 18	Daylight	2–3
	I. T I	5	4 5 39·8	± 2·3	104·7	+ 9	Twilight	3
	II. T E	5	4 37 46·8	± 4·1	105·0			2–3
	I. S I	5	5 2 10·0	± 1·8	99·0			2
19	I. E R	First speck	4 28 56·8				Faint twilight	3
	,,	4	4 30 32·6				Poor	3
25	II. T I	3	4 38 32·3		[100·0]	+ 15	Last 3 phases only caught	2
Aug. 2	I. O D	4	5 17 49·0		[90·0]		Low, very poor. Power 250 non-ach.	4

The column "Estimated Latitude" indicates the observer's eye-estimate of the latitude on Jupiter at which the phenomenon took place. These estimates are necessarily very rough.

Notes.

1909 Mar. 30. IV S.E. Only the last two phases ($\frac{3}{4}$ out and last contact) were observed, as the phenomenon occurred much earlier than predicted. The assumed semi-duration was 234 secs/cos 40°. IV was very dim.

Apr. 15. IV T.I. IV is always dim, and is really too faint for a 9-inch aperture; nevertheless, on more than one occasion a N. polar brightening has been seen, and this bright spot, like that of III, is somewhat N.f. When IV was entering on the disk its altogether-brightness was much the same as that of the contiguous limb of Jupiter; but when approaching internal contact, a small bright spot, brighter than the limb, was plainly seen against the sky.

May 2. IV in transit was a dark-grey spot, but it was not as dark or as large as the shadow of III, which was also on Jupiter's disk. When the transit was ending, IV was still visible as a dark spot on Jupiter's limb. At middle-phase (IV half on and half off) the illumination of the part against the sky was very dim, whilst the part on the limb was dark-grey.

May 5. This phenomenon was fully observed and recorded by moonlight, no artificial light being necessary (local time about midnight).

June 15. II T.E. This was lost. On going to the telescope 6 mins. before the predicted time it was found that the satellite was already clear of Jupiter's disk by about one minute of time.

Aug. 2. A non-achromatic eyepiece was used, and Jupiter so placed in the field that the want of achromatism in the eyepiece compensated the atmospheric dispersion, thus a practically colourless image was obtained.

Comparison with the Ephemeris (*Nautical Almanac*).

The following table exhibits a direct comparison, but it must be borne in mind that the Nautical Almanac only predicts the occultations, transits, and shadow-phenomena to the nearest whole minute. When this is remembered, it will be apparent from the comparison that the contact phenomena can be observed with an accuracy little if at all inferior to the eclipses. But whilst the times of central phase of the contact phenomena are free from a progressive change, depending on the size and power of the telescope used, the eclipses are not. It is for the latter cause that the photometric observations of eclipses have been made at Harvard and Paris. As five different contact phenomena (or thereabouts)

can be observed to one eclipse, their probable errors may be more than twice as great in comparison to give results of equal value. Theoretically, the contact phenomena are simpler than the eclipses, as the latter depend on Jupiter's shadow-cone, modified from a geometrical cone by refractive and penumbral effects as far as Jupiter is concerned, and by other difficulties connected with the reflective powers of the satellites themselves. The shape and size of Jupiter's disk can be determined very exactly from contact phenomena, and it is tempting to start on this; but, as mentioned before, we await Professor Sampson's theory.

Woolhouse's tables used by the Nautical Almanac are very rough, and do not distinguish between the illuminated and dark portions of Jupiter's disk.

Comparing the eclipse errors of I in both 1908 and 1909, it is obvious that the light-equation used by Damoiseau (493·2 secs.) is too small; my observations alone show that the time requires to be increased by about 13 secs., but this figure includes imperfections of the theory of I. The comparison of II indicates that its theory is considerably defective.

The theories of all four satellites suffer through the omission of what may be called the solar-impressed excentricity, which is of considerable importance in the case of IV. It is not easy to see how this perturbation was overlooked by the earlier investigators, as considerations of symmetry indicate it; in fact, the Sun in the theory of the four large satellites can be considered as a fifth satellite disturbing the other four.

SATELLITE I.

Obs. – N.A.

1909.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.	Angle. Sun-Jup.-Earth. °
Feb. 1					+0·6	−2·6	+2·1	−1·6	5·3
13	+0·4								3·0
14								−1·5	2·8
Mar. 1	near J	−0·3	[+1·5]						0·2
9					+1·3				1·8
15			+0·1						3·0
16					+0·8	−1·4	+1·2	−2·8	3·2
17		−0·7							3·4
23					+0·4		+1·3		4·5
24		−0·6							4·7
25						−2·1		−3·2	4·9
31			+1·0						5·9
Apr. 1					+1·1	−2·6	+0·8		6·1
2		−0·5							6·3
8					+1·4	−2·8	+1·6	−3·3	7·0

SATELLITE I.—*continued.*

1909.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.	Angle. Sun-Jup.-Earth. °
Apr. 9		−0·5							7·2
10								−4·2	7·4
15					+1·1		+1·9		8·2
16		−0·7	+0·7						8·3
17							+1·9	−4·1	8·5
23			+0·6						8·9
24					+2·3	−1·7	+2·0	−3·8	9·1
25		−0·5							9·2
30			+0·2						9·6
May 1					+1·4		+2·3		9·8
2		−0·6	+1·1						9·8
3								−4·8	9·9
8					+2·5				10·2
9		−0·4	+0·7						10·3
10						−1·2	+1·4	−4·5	10·4
16			−0·4						10·6
18		−0·5							10·6
24					+2·9		+1·6		10·7
25		−0·6	+0·5						10·7
26						−1·7		−4·3	10·7
June 1			+0·6						10·7
2					+3·6	−2·0	+1·3	−4·5	10·7
3		−0·5							10·7
8			−0·6						10·6
9					+2·6				10·6
10		−0·5							10·5
16					+2·6				10·3
17			−0·0						10·2
18						−2·1		−4·4	10·2
24			+0·3						9·9
25					+2·7	−0·9	+1·6		9·8
26		−0·4							9·7
July 2					+2·8		+3·4		9·3
3		−0·3	−0·2						9·2
10			+0·4						8·6
11						−2·0		−3·9	8·5
18					+1·7		+2·2		7·9
19		−0·3							7·8
Aug. 2			[−1·2]						6·3

SATELLITE II.

Obs. – N.A.

1909.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.	Angle. Sun-Jup.-Earth. °
Jan. 30	+1·7			−1·0					5·8
Feb. 24				−2·3					0·9
Mar. 26					+1·2				5·0
28		−1·4							5·4
Apr. 11			+1·1						7·6
13						−3·3	+2·2	−6·2	8·0
22		−1·3							8·9
27					−0·7		+1·3		9·4
29		−1·5							9·5
May 6			+1·2						10·1
13			+1·5						10·5
15						−7·5	+1·3	−6·6	10·5
22					−4·0	−9·4	+1·0		10·7
24		−1·4							10·7
31		−1·3							10·7
June 14			+0·9						10·4
16							+2·0	−5·7	10·3
23					−7·2	−11·3	+1·6		10·0
25		−1·3							9·8
July 2		−1·0							9·3
18						−10·2	+1·6		7·9
25					−5·5				7·2

Satellite III.

1909.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.	Angle. Sun-Jup.-Earth. °
Feb. 1	+2·9								5·3
Mar. 16			+1·8						3·2
27						−4·9	+1·3	−5·3	5·3
Apr. 3					+1·4	−4·2	+1·3	−7·1	6·4
14		−0·3							8·1
May 2								−8·9	9·8
5			+1·2						10·0
9							+1·5	−9·3	10·3
16					+3·6	−4·0	+1·8		10·6
23					+3·1				10·7
27		+0·4							10·7
June 3	+3·2			−0·1					10·7
10			+0·8						10·5
14								−8·0	10·4
21							+2·4		10·1
28					+2·7				9·6
July 9		+0·5							8·7
16				−1·5					8 1

Satellite IV.

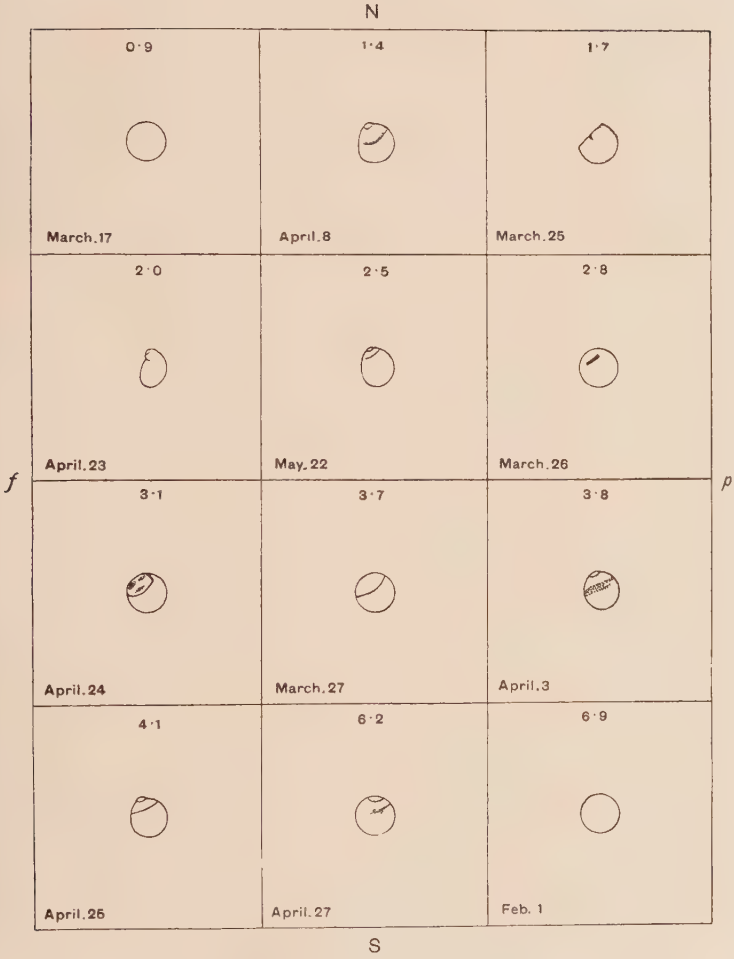
1909.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.	Angle. Sun-Jup.-Earth. °
Jan. 30	+2·5								5·8
Mar. 13							−4·0		2·4
30								−12·3	5·8
Apr. 15					−3·5				8·2
24	+1·5	−0·8							9·1
May 2						−7·8			9·8
19							−3·2		10·6
27			−1·8						10·7
June 5								−14·0	10·6
13				+2·0					10·5
30	−0·1								9·4

Disks of the Satellites.

The observations made on III follow. At no time was any peculiarity noticed with regard to I and II, which showed uniformly illuminated round disks. IV is too dim with 9-inches and a power sufficient to show a good disk,—on two occasions during transit notes were made, and are given on a previous page.

Near quadrature, or rather not near opposition, the oblong shapes of the shadows of all the satellites at entry and exit could not be overlooked. That the shadows should be oblong when looked at obliquely is sometimes forgotten; thus it is stated that on 1907 Mar. 30, at $12^h\ 55^m$ G.T., Professor Lowell saw the shadow of I to be nearly twice as broad as it was high (*Nature*, 1909 May 20). Jupiter was in quadrature on 1907 Mar. 23 and on Mar. 30; shadow ingress was predicted for $12^h\ 51^m$; thus when Professor Lowell made his observation, the perspective elongation of the shadow, an ordinary geometrical effect, was about a maximum.

Observations of the shape of the disk of III and of its markings were made on 64 nights. These are entered in order of age counted from the time of superior geocentric conjunction. (I now believe that it would have been better to have taken heliocentric instead of geocentric conjunction.) The shape and markings were sufficiently well seen on 10 occasions to warrant sketches. These are produced on Plate 5, and the remarks made last year, vol. lxix. p. 534, might be repeated. The sketches are in good agreement with those of 1908, but at no time in 1909 was the irregular shape or bright spot so conspicuous as in the previous season. There can be no doubt that the satellite rotates and revolves around Jupiter in the same time, but it is very remarkable that the dark marking, which causes III to appear 9 day-moon-shaped during ages 1^d to 3^d never transits the disk so as to cause a similar appearance on the preceding side; it is also very seldom that the N. polar white spot is far from Pos. Ang. 45°, either marginally or in the n.f. quadrant of the disk. With more light than a 9-inch gives, I believe that the whole disk of III would always be seen and that the N. polar spot would always be visible; the visibility of the spot, however, varies greatly; it is easiest at ages 1^d to 3^d, but has been seen to 6^d. The N. tropical belt on III seems to be complete over more than a semicircumference, as it is seen from ages 1^d to 6^d. There is probably a meridian zone of matter which gets black in the absence of sunlight, but which is rapidly altered and becomes reflective when the Sun rises on it; in this way only does it seem possible to account for the terminator shown during ages 1^d to 3^d.



SATELLITE III. OF JUPITER, 1909.

Sketches arranged in order of days since superior Geocentric Conjunction.

SATELLITE III.

Age. d	1909.			Def.
0·1	June	3	Round, N. polar spot and its dark margin glimpsed occasionally whilst waiting for E.D.	3
0·2	Apr.	14	Round.	3
0·3	May	27	Round, N. polar spot glimpsed.	3
0·4	July	2	Round.	3
0·6	Mar.	31	Round.	3
0·6	June	25	Round, N. part of disk is brightest.	1
0·7	Feb.	2	Round.	4
0·7	May	6	Round, perhaps flattened N.f.	2
0·7	,,	13	Round, or very nearly so.	2
0·9	Mar.	17	Round.	4
1·0	Apr.	29	Round.	3
1·3	,,	15	Shape of Moon 10 d. old, term. away from J., N. bright spot and mark to S. of it seen, but not plainly.	2
1·4	,,	8	Shape of Moon 10 d. old, term. away from J., marginal bright N. spot and mark seen. [Sketch.]	2
1·4	May	21	Shape of Moon 8 d. old, spot at N. pole and marking close up. Glimpses only.	3
1·5	June	26	Shape of Moon 10 d. old, N. polar spot and dark marking seen, but not easily.	2
1·5	May	14	Shape of Moon 9 d. old.	2
1·6	,,	,,	Shape of Moon 9 d. old, bright spot marginal at N. pole, and surrounding dark mark in latitude 60° N.	2
1·7	Mar.	25	Dark mark on limb at 45° (N.f.) and disk slightly flattened, no bright spot. [Sketch.]	3
1·7	May	7	Shape of Moon 10 d. old, cut off on f. side, bright spot N.f. 30°, disk somewhat kidney-shaped.	2
2·0	Apr.	23	Somewhat kidney-shaped, cut off on N.f. side, bright spot just glimpsed within small N. part, but it is marginal N.f. [Sketch.]	3
2·1	,,	30	Shape of Moon 9 d. old, term. away from J.	4
2·1	June	5	Shape of Moon 8 d. old, term. away from J.	3
2·2	Apr.	16	N.f. segment cut off, N. polar spot marginal and very small.	2
2·4	July	4	Oval, dark mark encroaches on N.f. part of limb.	3
2·5	May	22	Shape of Moon 10 d. old, spot N.f. and mark close up to it.	3
2·5	,,	15	Shape of Moon 11 d. old, dark marking N.f., no bright spot.	2

SATELLITE III.—*continued.*

Age. d	1909.			Def.
2·6	May	15	Perhaps spot marginal at N. pole and mark close up?	2
2·6	Apr.	2	Round, shading in N.f. quadrant of disk.	3
2·8	Mar.	26	Whole disk seen, a well-marked darkish streak in N.f. quadrant. [Sketch.]	3
2·9	May	1	Nearly round, marking towards N. pole.	3
2·9	,,	8	Round, nothing certainly seen on disk.	2
2·9	June	13	Round. N. spot glimpsed?	2
3·1	Apr.	24	Nearly or quite round, but somewhat cottage-loaf appearance, N.f. quadrant is occupied by a brightish spot surrounded by a dark curve. [Sketch.]	1
3·2	,,	17	Figure-of-eight?	5
3·5	June	28	Round, N. polar spot seen well within disk at 30° (N.f.).	2
3·5	May	23	Round, just before T.I.	4
3·5	Apr.	10	Round or nearly so, spot and mark seen because known, angle through spot and centre of III = 30° (J. polar axis = 0° – 180°).	2
3·5	,,	3	Round, small brightish spot enclosed by shaded curved line—inconspicuous; just before T.I.	2
3·6	,,	,,	Transit ending, remarkable, see *Ast. Nach.*, No. 4324.	1
3·6	May	16	Round.	2
3·7	Mar.	27	N. tropical line of latitude well seen; there is the bright spot within it, but it is inconspicuous, and might even be due to contrast with the surrounding marking. [Sketch.]	2
3·7	May	9	Round.	3
3·7	Feb.	5	Shape of Moon 10 d. old, dark marking N.f., term. to J., tilt 30°.	3
3·8	,,	,,	Later, better seeing, nothing certain, only suspicions.	2
3·8	Apr.	3	Spot more conspicuous than earlier in the evening, is at 45° (N.f.) very white, almost marginal.	2
3·8	May	9	Round, indistinct markings?	2
3·9	,,	2	Nearly round, spot and mark in N.f. quadrant.	3
4·1	Apr.	25	Round, but bright spot distinct; it seems to project from N.f. limb, so that at first glance the satellite looks oblong, dark curve bounds spot. [Sketch.]	2
4·4	,,	11	Round.	3
4·6	Feb.	13	Round.	3
4·7	May	10	Round, indistinct markings.	3
4·8½	Mar.	28	Round.	

SATELLITE III.—*continued.*

Age. d	1909.			Def.
4·9	May	3	Round, spot and mark barely discernible on N.f. margin.	2
5·0	Jan.	30	Quite round, no sure markings.	2
5·1	Apr.	26	Round, nothing certainly seen on disk.	3
5·3	,,	19	Round, no bright spot.	3
5·3	June	1	Round, N. hemisphere perhaps brightest part of disk.	2
5·5	May	25	Round, perhaps N. polar spot glimpsed?	3
6·2	Apr.	27	Round, nothing very distinct, but bright spot is at N. pole, or slightly preceding, instead of following as is usual; dark marking glimpsed. [Sketch.]	2
6·3	Mar.	1	Round.	3
6·3	June	2	Round.	3
6·4	Apr.	13	Round.	3
6·5	May	26	Round.	2
6·7	Mar.	30	Round.	3
6·8	June	24	Round.	2
6·9	Feb.	1	Quite round, no sure markings.	3
6·9	Mar.	24	Round.	2
6·9	May	5	Round.	2
7·1	Apr.	28	Round.	3
7·1	May	5	Round, nothing seen on disk. At occultation III was dimmer than limb of J.	3

Johannesburg:
1909 *August.*

Stars of Spectrum Type O. By W. S. Franks.

In the course of a recent investigation on star colours and spectral types, it was found that the stars in the spectrum group O did not give so decided a colour position as in all the other groups. Thinking that this might be due to the small number included (8 out of a total of 2641 stars), I made a list of all the O stars in the "H.R.," 26 in number, for further study. According to their spectra, they are divided into three sub-classes:—

I. Oa, Ob, Oc. Bright bands on faint continuous spectrum.
II. Od, Oe. All lines dark except two bright bands.
III. Oe5. All lines dark; intermediate class between O and B

There are 6 stars in I, 5 in II, and 15 in III, = total 26, out of 9110 stars.

The O type is antecedent to B in the usual sequence, and, though numerically insignificant, is possessed of more than ordinary interest. Like type B, it has a high proportion of bright stars, and it also exhibits a strong affinity for the Galaxy—in fact, *all* its stars are situated in, or on the borders of, the Milky Way, as will be seen from the following table:—

Star.	R.A. (1900). h m	Decl. ° ′	Mag.	Spect.	Colour.	Remarks.
Persei	3 52·5	+35 30	4·05	Oe 5	Y^1	Fr. (4 catalogues); P=O.
Orionis	5 29·6	+ 9 52	3·66; 5·56	Oe 5	O	,, (3), Y^1; P=Y^1. Double, Σ738.
,,	,, 30·4	− 5 27	5·36	Oe 5	R^1	,, (2), Y^1. Trapezium in M 42. Multiple, Σ748. Spectroscopic binary.
,,	,, 30·5	− 5 59	2·87	Oe 5	O	,, (4), Inv. in neb. H V 31. Double, Σ752. Spectroscopic binary.
(15) Monocerotis	6 35·5	+ 9 59	4·68	Oe 5	G^1	,, (2), O. Inv. in neb. H V 27. Double, Σ950. Spectroscopic binary.
Canis Majoris	,, 50·0	−23 48	6·55	Ob	O	,,
,,	7 14·5	−24 23	4·90	Oe	Y^2	,, (2)
,,	,, 14·5	−24 47	4·40	Oe 5	Y^1	,, (2) Triple (h 3948). In cl. H VII 17.
Puppis *	8 0·1	−39 43	2·27	Od	O	N.S. Wales section; Williams, O.
Velorum †	,, 6·5	−47 3	2·22	Oa Pec.	O	N.S. Wales section; Tupman, O; Eddie, V^1. Triple Δ 64, 65.
Carinæ	10 37·4	−59 9	6·48	Oc Pec.	—	
Crucis	12 50·1	−56 17	5·58	Oe 5	—	
Muscæ	13 1·7	−64 46	5·64	Oa Pec.	Y^1	Russell. Double (h 4568) Δ 129.
Circini	15 8·9	−60 35	5·24	Oe 5	O	Dunlop.
,,	,, 10·8	−60 8	5·50	Oe 5	—	
Scorpii	16 44·5	−41 4	5·37	Oe	G^1	Dunlop. In cl. N.G.C. 6227.
,,	,, 45·3	−41 41	6·56	Oc	—	
,,	,, 47·3	−41 40	6·72	Oa	—	Very near cl. N.G.C. 6231.
,,	,, 48·0	−41 0	6·03	Oe	—	
Sagittarii	17 57·7	−24 22	5·86	Oe 5	Y^2	Fr. Involved in neb. M 8.
Cygni	20 53·1	+44 33	6·01	Oe 5	O	,, ; P=Y^1. Inv. in great neb. H V 37.
,,	21 14·8	+43 31	5·06	Oe 5	O	,, (2); P=Y^1.
xxi. 248 Cephei	,, 35·9	+57 2	5·64	Oe 5	Y^1	,, ; De, O; P=Y^2. Triple, Σ2816.
,,	22 2·1	+61 48	5·17	Oe 5	Y^1	,, O; P=Y^1. Double, β 697.
,,	,, 8·1	+58 56	5·19	Od	$Y^{1\cdot5}$	,, Y^2 (2), Y^1; P=Y^1.
Lacertæ	,, 34·8	+38 32	4·91	Oe 5	O	,, (2); P=O. Double, $Σ^1$ (2739) c.g.

* ζ Puppis. The star from which the secondary series of hydrogen was discovered ("H.R.").
† γ Velorum. Spectrum very peculiar—brightest star of type ("H.R.").

The colour notation is that given in *Monthly Notices*, xlvii. 269, where O = white; R^1, ruddy white; Y^1, yellowish white; G^1, greenish white; V^1, purplish white; Y^2, pale yellow, etc.

We may notice several peculiarities in the above list. Two-thirds of the objects are south of the equator; and 10 of them are arranged in 5 wide couples (bracketed together), a rather curious feature; and all of them are in or near the Galaxy, indicative of a real connection. Five of the stars are involved in nebulæ and 2 in clusters; 3 are spectroscopic binaries, and 7 others are doubles; and 3 of them have "peculiar" spectra. Now as to their colours, of which 20 are recorded. The numbers enclosed in parenthesis refer to my four catalogues: P = Potsdam, and De = Dembowski. The *mean* colour has been taken; then classing G^1 with O, R^1 with Y^1, and $Y^{1\cdot5}$ with Y^2, we have 11 = O, 6 = Y^1, and 3 = Y^2—showing a regular diminution from white to pale yellow. We can now place the colour corresponding to the *maximum* in each spectral group in a proper sequence, which the adoption of mean colour values has rendered still more striking. Thus:—

Spectrum.	Normal Colour.	Symbol.	Spectrum.	Normal Colour.	Symbol.
O	White	O	G	Pale yellow	Y^2
B	do.	O	K	Yellow	Y^3
A	Wh. to yelsh. wh.	O–Y^1	K5	Pale orange	Or^2
F	Yelsh. wh.	Y^1	M	Orange	Or^3
F5	Do. to pale yel.	Y^1–Y^2	N	Orange-red	OrR^3

Uxbridge:
1909 *September* 1.

Note on the Cyclones of the Indian Ocean 1856–1867, *and their Association with the Solar Rotation.* By E. Walter Maunder.

Five years ago I had the honour of submitting to the Royal Astronomical Society a paper on "Magnetic Disturbances, 1882 to 1903, as recorded at the Royal Observatory, Greenwich, and their Association with Sun-spots." In that paper I demonstrated that magnetic disturbances tended to recur at intervals corresponding to one or more complete synodic rotations of the Sun, with a frequency much greater than could be ascribed to chance. It followed, therefore, that the magnetic disturbances experienced on the Earth are intimately connected with the solar rotation. Their exciting cause must lie in the Sun, but the action, whatever its nature, bringing about those disturbances, does not proceed from the surface of the Sun as a whole, but from restricted portions of that surface. Nor is it radiated equally in all directions, like light or heat; the action is confined to a definite and restricted direction. The solar influence is conveyed to the Earth in definite streams; streams which, continually supplied from the same areas of the Sun's surface, appear to us at our distance to be rotating with the same angular speeds as the areas from which they rise.

The discovery of the "Interval-Relation," as for the sake of brevity I termed the above tendency observed in magnetic disturbances, naturally led me to inquire whether any meteorological phenomena tended likewise to recur after the expiration of a solar rotation period. It is, of course, obvious that the Sun has a most powerful effect upon the meteorology of the Earth by its radiation of light and heat; that terrestrial meteorology is, in chief, a function of the changes in the presentation to this solar radiation of the different regions of the Earth's surface. But it seemed to me worth while to inquire whether there was any evidence in terrestrial meteorology of an "Interval-Relation" analogous to that shown by magnetic disturbances.

If such a relation exists, it will be no easy matter to demonstrate it, for the following reasons:—

1. As already pointed out, the solar influence upon terrestrial meteorology is in chief a function of the changes in the presentation to the solar radiation of the different regions of the Earth's surface. Any action from limited regions of the Sun and along restricted lines might be expected to be wholly subordinate to the effect of the general solar radiation, and hence the evidence of such action would be liable to be entirely masked by the effects of season and of climate.

2. It cannot be expected that the meteorology of the whole Earth should respond to the impact of a stream-line from the Sun, in the same corporate way that the Earth's magnetism does. The hemisphere in darkness may be expected to escape partially, if not wholly, whatever action such an impact may set up. Since, therefore, the synodic rotation period of the Sun is in the mean about 27 days 7 hours, we should expect that there would be few cases in which an effect would manifest itself in three successive rotations. In other words, the sequences would tend to be interrupted; the disturbances in any particular locality would tend to recur at the end of two or three rotations of the Sun, rather than at the end of one.

3. It would only be in the case of the solar stream-line passing over a region where the meteorological conditions were in a state of unstable equilibrium that we could expect any noticeable result to take place.

4. Whilst a magnetic disturbance, if sufficiently intense, manifests itself instantaneously over the entire planet, a meteorological disturbance will probably take a very appreciable length of time to develop.

5. It is a matter of no small difficulty to decide the exact time when any given meteorological disturbance had its beginning.

6. The number of striking meteorological changes is so great that it is only under very exceptional circumstances that a record is afforded of sufficient simplicity for the "Interval-Relation,"—if it really exists,—to become evident.

7. On the other hand, inasmuch as it is only an interval that we have to detect, and not the exact moment of the commencement

of the disturbances, any completely systematic error in determining such commencement is of no consequence at all: the interval is not affected by it.

Clearly, if the "Interval-Relation" is to be detected at all, the search for it must first be made in the records of those regions—such as the tropics—where the meteorology is comparatively simple. It therefore occurred to me to examine such lists of tropical cyclones as were readily accessible to me, in order to ascertain whether or no they manifested this relation.

Father Algué's most valuable work on the Cyclones of the Philippine Islands did not prove suitable for a preliminary inquiry. The cyclones recorded were too numerous, and the dates when they took their rise out in the Pacific much too uncertain. A careful examination of the catalogue left the question undecided: there were many cases of the apparent recurrence of a cyclone at the end of a solar rotation, but it was not clear that this happened much more frequently than might have been expected from a chance distribution.

Sir John Eliot's Catalogue of the Cyclones in the Bay of Bengal * dealt with a case which was better adapted for the present inquiry as the conditions were simpler, and the "Interval-Relation" did appear to be presented in it with a frequency which was distinctly greater than would be expected if there were no determining cause at work tending to produce an interval of about 27 days.

But a list of cyclones in the Southern Indian Ocean, compiled by the late Dr. Meldrum,† the Director for so many years of the Royal Alfred Observatory, Mauritius, dealing as it did with a very wide stretch of ocean, and,—from the peculiar position of Mauritius,—giving a fairly complete presentment of all the great atmospheric commotions within that area, was much better adapted to a preliminary inquiry. And on examination it was found to present several striking instances of the recurrence of a cyclone at the interval of the solar synodic rotation.

The years covered by the catalogue were the years 1856–1867, and 110 cyclones are included. Of these I have used 109, the last cyclone in the catalogue—1867 December 15—being omitted, as it belongs to a new cyclone season, the cyclone season in Mauritius being limited to the eight months October to May.

The longest succession of cyclones following each other at intervals about equal to the solar rotation is the following:—

1865 December 8
1866 January 6
,, February 2
,, March 3
,, March 30

* *Handbook of Cyclonic Storms in the Bay of Bengal, for the use of Sailors*, vol. i., by John Eliot, M.A., 2nd edition, published by the Meteorological Department of the Government of India, Calcutta, 1900, pp. 164–188.

† "On Cyclone and Rainfall Periodicities in connection with the Sun-spot Periodicity," by Charles Meldrum, *British Association Report*, 1874, pp. 218–240.

It will be seen that if we suppose that the actual commencements of the storms of January 6 and March 3 had really fallen a little before the midnights of January 5 and March 2 respectively, but were not recorded until after those midnights, an error which might be merely due to the scattered positions of the shipping in the Indian Ocean, or even to a failure to allow for difference of longitude, the sequence would be absolutely regular. Even as it stands, the probability that the sequence is wholly accidental is exceedingly small, for it should be noted that no other cyclones were catalogued at Mauritius during the half-year 1865 October to 1866 March.

But this sequence does not stand alone. In 1862 the following succession occurs:—

1862 January 2
„ January 28
„ February 23
„ March 23

This sequence is less striking than the first given, inasmuch as there were several other cyclones during the same period. Indeed a third sequence of three items ran its course during the same season:—

1861 December 26
1862 January 23
„ February 21

A single sequence like the first of those given above, though a proof of special action, does not of itself afford proof that that action is connected with the solar rotation. It might conceivably be connected with the Moon, either with the draconitic month of 27·21 days, with the sidereal month of 27·32 days, with the anomalistic of 27·55 days, or with the synodic month (or lunation) of 29·53 days. The first three quantities differ very slightly from the mean synodic rotation period of the Sun of 27·26 days, and the fourth accords with the rotation period given by some of the higher latitudes within the sun-spot zones.

But though these various months approximate so closely in length to the synodic rotation of the Sun, any influence which we can ascribe to the Moon must in one important feature differ entirely in its effect from any influence that we can connect with the solar rotation. The influence due to the Moon must be strictly *periodic* in character, for it must vary with the motion of the Moon in its orbit—it must be a function of it—and that motion is strictly periodic. On the other hand, an influence depending upon the synodic rotation of the Sun cannot (*in the main*) be periodic in character.

Any action which arises from the whole surface of the Sun, and is equally powerful from every part of that surface, cannot produce effects giving us any indication of the solar rotation. It is only when we have an irregularity in such action, when there are certain regions that are more or less active than the general surface,

and when such irregularity is continued for long periods of time, that we can have the rotation period brought out. A single meridian of the Sun—or what comes to nearly the same thing, a system of meridians which can be represented permanently by a single fixed meridian as their resultant—must thus be continuously differentiated from the rest of the surface. This is a condition which finds no existence in nature. Out of some 6000 groups of sun-spots observed at Greenwich in 33 years, only 468 were observed to have a continuous existence into a second rotation, 118 into a third, 25 into a fourth, 12 into a fifth, and in one instance only, and that somewhat doubtfully, did a spot group survive to be seen in a sixth apparition.* And these recurrent groups were found in every possible longitude. No single meridian of the Sun is distinguished from the rest for any long duration; consequently, if we take observations of any kind and subject them to "periodic analysis," in order to obtain evidence of an action connected with the solar rotation, we shall inevitably fail to find a period; or, if we find one, it will be only fictitious, due to some chance grouping of the meridians most active during the time covered by our scrutiny. This consideration is sufficiently obvious, yet it is a remarkable fact that during the last fifty years much labour has been devoted to analysing meteorological or magnetic observations with a view to finding a true period in them that should correspond to that of the synodic rotation of the Sun. If such a period had resulted from such analysis, it would have gone far to prove that the effect thus detected could not be due to the Sun, though it might possibly be due to the Moon.

In the present case, although the observations run for only twelve years, it is clear there is no close connection between any action of the Moon and these Mauritius cyclones. Dr. Meldrum's Catalogue given in Table I. shows 109 cyclones in the twelve years 1856–1867. The first column of Table I. contains a reference number; column 2 gives the date of commencement of the cyclone; column 3 states its character, as to whether it was "progressive," *i.e.* whether it moved on the Earth's surface, or was "stationary"; column 4 gives the number of the rotation of the Sun, following Carrington's numeration, as in the Greenwich Photo-heliographic Results, according to which rotation No. 1 began 1853 November 9, and adopting 25·38 days for the sidereal rotation period of the Sun; column 5 gives the number of days and decimal of a day that had elapsed of the rotation at the commencement of a storm, the storm being supposed to have commenced at 0 hours Greenwich civil time of the date given, or at 6 hours civil time for longitude 90° east. The four remaining columns give respectively the age of the Moon, its declination, latitude, and semidiameter for the same time. Table II. gives a similar catalogue for the cyclones in the Bay of Bengal for the same years. The third column,

* "Catalogue of Recurrent Groups of Sun-spots, 1874–1906," by Mrs Annie S. D. Maunder, Appendix to *Greenwich Observations*, 1907, p. 6.

however, indicates the intensity of the storm; F, meaning *Feeble*; M, *Moderate*; C, a *Cyclone*; and H, a *Hurricane*.

Table III. shows the distributing of the Mauritius and Bengal cyclones respectively, according to the age of the Moon; and it will be seen that their distribution is fairly even throughout the month. So, again, there is no very marked tendency for Mauritius cyclones to be more numerous when the Moon is south of the line than when she is north of it, 50 of the 109 cyclones having taken place when the Moon's declination was northern, and 59 when it was southern. The same distribution is noticed if the latitude of the Moon be taken, the northern latitudes being 51 in number to 58 of the southern. Nor does the distance of the Moon appear to have any great effect; the cyclones occurring when the Moon had a semidiameter smaller than the mean (that is to say, when she was further from the Earth than the mean distance), being 62 as opposed to 47 when she was nearer the Earth than the mean distance. These differences appear to be simply due to the shortness of the time covered by the catalogue, and to the varying speed of the Moon in her orbit. It is clear, therefore, that no noticeable effect can be ascribed to lunar action, since there is little or no difference in the number of cyclones whether the Moon be near the Earth or far from it, whether she be in conjunction or opposition, or whether she pass near the zenith of the region considered or remain remote from it.

The case is entirely different if we examine the relation of the dates of beginning of the cyclones as referred to the Sun's rotation; in other words, if we search for an interval, and not for a period. Table IV. gives the cyclones of Tables I. and II. arranged in sequences according to the "day of rotation" which appears in column 5 of those tables. As a rule, the sequences have been limited to intervals of not more than three rotations, but in cases where a sequence was already indicated a cyclone occurring at a distance of four or even five rotations from the beginning or end of that sequence has been included in it. About 70 per cent. fall into one or other of these sequences.

It will be noticed that a cyclone does not recur always at the end of a single rotation; the tendency is rather for the recurrence to take place at the end of two or three rotations. Now, as the cyclone season is limited, generally lasting only for five months at Mauritius, it is clear that there are many cases where no return was possible. There were 82 possible cases of the return of a cyclone at the end of one rotation, and 15 returns are registered. But there were 31 returns at the end of two rotations out of a possible 69, and 30 returns at the end of three out of a possible 50. These last two numbers are considerably higher than would be expected on a chance distribution.

The recurrence after two or three rotations is exactly what might naturally be expected if the cyclones were partly due to the effect of a stream-line from the Sun striking the Earth. For, seeing that the mean synodic rotation of the Sun is about $27\frac{1}{3}$ days,

a region presented to the Sun on the first occasion of a stream-line overtaking the Earth would probably be turned away from it either on the second or third occasion, or on both.

It is worth bearing in mind,—if the evidence supplied by Table IV. be accounted sufficient to render probable that the synodic rotation of the Sun is manifested by the intervals between the Mauritius cyclones,—that it does not necessarily follow that the cyclones are directly connected with sun-spots. As I have often had occasion to remark, sun-spots are the class of solar phenomena which are most easily observed, and of which we have the most continuous record. We are almost obliged, therefore, to go first to our sun-spot observations if we wish to correlate any terrestrial changes with changes on the Sun. But this condition does not imply that sun-spots are in themselves the most significant of solar phenomena.

So, again, it does not follow if cyclones show the same "Interval-Relation" as magnetic disturbances, and therefore are recognised to be in certain cases excited by stream-lines from the Sun, that the stream-lines are identical for the two phenomena, or that the particles composing them take the same time in traversing the distance to the Earth. It is also unlikely that a cyclone can be developed with anything approaching the same instantaneousness that characterises a magnetic storm. However frequently the excitement of a cyclone may follow the impact of a solar stream-line, it is not probable that any very simple or obvious relation will be discovered between a cyclone and a magnetic storm, or the appearance of a great group of spots on the solar disc.

The evidence offered by the Mauritius cyclones would be strengthened if it were possible to show that the cyclones occurring north of the equator during the quiet season for Mauritius fell into the same sequences—that is to say, were continuations of them. It will be seen from Table IV. that the cyclones registered by Sir John Eliot for the Bay of Bengal do, as a matter of fact, supplement the Mauritius sequences in quite a noticeable number of cases, every year in the Bengal catalogue being represented in Table IV. except 1856, 1862, and 1865, when there was only one feeble Bengal cyclone in each year, and 1866, when there was none at all. This is the more remarkable since the area of the Bay of Bengal is so much more restricted than that of the great Southern Ocean, and the cyclones recorded are not one-fourth in number those of Dr. Meldrum's catalogue.

A full confirmation of the case would be afforded if the intervals in the sequences of the Mauritius and Bengal cyclones were filled up by cyclones occurring in the Atlantic or Pacific. Hitherto I have not come across records that would enable this point to be brought out. If such a comparison could be made in a satisfactory manner, and it were found that cyclones the world over tended to form parts of the same sequences when referred to the solar rotation, the demonstration of their connection with that rotation would be very complete. In the actual state of our

knowledge, however, I would not claim more for the present paper than that it suggests a case for inquiry, and the lines upon which such inquiry should proceed.

In considering the evidence of dependence upon the solar rotation given by the sequences in Table IV., which can be largely supplemented from the records of other years, it is necessary to bear in mind that, though many of the sequences given are probably only accidental, yet unless *all* the sequences are accidental a connection with the solar rotation must be admitted. It is not possible to admit that one sequence is due to the solar rotation unless we admit at the same time that the solar rotation can play, and does play, some part in bringing about these atmospheric disturbances.

It seems to me that the evidence afforded by Table IV. is sufficiently strong to make a case for a searching inquiry as to whether the records of other years and other localities show similar features; and it is with this purpose,—to afford a ground for such further research,—that I offer the foregoing note.

69 *Tyrwhitt Road*,
London, S.E.,
1909 *November* 6.

TABLE I.

Cyclones in the South Indian Ocean.

Reference No.	Date of Commencement of Cyclone.	Character.	No. of Rotation.	Day of Rotation.	Age of Moon.	Dec. of Moon.	Lat. of Moon.	Semi-diameter of Moon.
				d	d	° ′	° ′	″
1	1856 Feb. 1	P	30	22·0	24·0	S. 22·53	S. 2·43	924
2	Mar. 20	S	32	15·3	13·1	N. 10·33	N. 3·34	883
3	31	S		26·3	24·1	S. 25·42	S. 5·16	960
4	Apr. 4	P	33	3·0	28·1	S. 3·32	S. 2·27	1001
5	Nov. 7	S	41	2·1	9·1	S. 14·30	S. 3·38	963
6	12	S		7·1	14·1	N. 18·41	N. 2·34	997
7	1857 Jan. 28	P	44	2·1	2·0	S. 12·34	S. 2·49	968
8	Feb. 13	P		18·1	18·0	S. 4· 9	S. 0·16	893
9	Mar. 23	S	46	1·4	26·5	S. 17·30	S. 3·40	965
10	Dec. 4	P	55	12·2	17·3	N. 26·49	N. 4·35	987
11	26	P	56	6·9	9·5	N. 12·52	N. 2·54	973
12	1858 Jan. 12	P		23·9	26·5	S. 27·43	S. 5· 3	887
13	Feb. 24	P	58	12·2	10·1	N. 27·22	N. 4·43	971
14	Mar. 6	P.S.		22·2	20·1	S. 24· 2	S. 4·54	892
15	18	S	59	6·9	2·5	N. 14·38	N. 3·31	972
16	31	P		19·9	15·5	S. 13·40	S. 3·21	908
17	Apr. 2	P		21·9	17·5	S. 22·48	S. 4·42	894
18	18	S	60	10·6	4·0	N. 28·24	N. 5· 8	984

TABLE I.—*continued.*

Reference No.	Date of Commencement of Cyclone.	Character.	No. of Rotation.	Day of Rotation.	Age of Moon.	Dec. of Moon.	Lat. of Moon.	Semi-diameter of Moon.
				d	d	° ′	° ′	″
19	1858 Nov. 1	P	67	16·9	24·9	N. 6 41	S. 0·27	947
20	11	S		26·9	5·3	S. 26· 5	S. 3·46	887
21	Dec. 15	P.S.	69	6·3	9·6	N. 6·15	N. 3· 2	935
22	1859 Jan. 1	S		23·3	26·6	S. 26· 8	S. 5· 5	893
23	24	P	70	19·0	19·8	S. 7·35	S. 3·29	947
24	Feb. 16	P	71	14·7	13·0	N. 20·36	N. 2· 3	999
25	Mar. 7	P	72	6·3	2·2	N. 7·46	N. 3·32	921
26	17	P		16·3	12·2	N. 11·18	S. 0· 6	984
27	Apr. 5	S	73	8·0	1·6	N. 17·15	N. 4·40	943
28	17	P		20·0	13·6	S. 12·16	S. 4· 7	947
29	21	S		24·0	17·6	S. 27·18	S. 4·51	908
30	22	S		25·0	18·6	S. 27·50	S. 4·26	900
31	June 3	P	75	12·5	1·7	N. 27· 5	N. 3·44	989
32	Nov. 16	S	81	15·1	21·0	N. 20·32	N. 1· 3	970
33	Dec. 1	P	82	2·8	6·4	S. 13·44	N. 0·42	889
34	9	P		10·8	14·4	N. 25·13	N. 4·49	957
35	1860 Jan. 10	P	83	15·4	16·8	N. 18·36	N. 0·18	997
36	18	P	83	23·4	24·8	S. 25· 7	S. 4·59	924
37	25	P	84	3·1	2·0	S. 11·38	N. 1·21	885
38	Feb. 11	P		20·1	19·0	S. 10·40	S. 4·47	978
39	22	P	85	3·8	0·2	S. 7·53	N. 2· 6	884
40	28	P		9·8	6·2	N. 21·54	N. 5·12	914
41	Mar. 2	P		12·8	9·2	N. 27· 5	N. 3·40	954
42	18	P	86	1·4	25·2	S. 18·29	S. 0·12	888
43	Apr. 26	P	87	13·1	4·8	N. 26·10	N. 2·47	944
44	May 27	P	88	16·9	6·2	N. 11·30	S. 1·51	969
45	29	S		18·9	8·2	S. 0·52	S. 3·53	972
46	Dec. 4	P	95	17·2	21·0	N. 12·48	S. 2·17	958
47	1861 Jan. 18	P	97	7·5	6·9	N. 10·51	N. 5·14	888
48	29	P		18·5	17·9	N. 4·17	S. 3·51	984
49	Feb. 5	P		25·5	24·9	S. 25·54	S. 2·55	943
50	11	P	98	4·2	1·2	S. 6·14	N. 3·33	898
51	28	P		21·2	18·2	S. 12· 5	S. 5· 7	994
52	Mar. 12	P	99	5·9	0·4	N. 2·41	N. 4·35	887
53	28	S		21·9	16·4	S. 15·12	S. 4·58	1004
54	Apr. 4	P	100	1·6	23·4	S. 17·47	N. 1·23	915
55	12	P		9·6	1·7	N. 19·24	N. 4·35	886

TABLE I.—*continued.*

Reference No.	Date of Commencement of Cyclone.	Character.	No. of Rotation.	Day of Rotation.	Age of Moon.	Dec. of Moon.	Lat. of Moon.	Semi-diameter of Moon.
				d	d	° ′	° ′	″
56	1861 Nov. 4	P	107	24·9	1·3	S. 23· 9	S. 2·56	1002
57	Dec. 18	P	109	14·3	15·9	N. 23·29	N. 0· 4	901
58	26	P		22·3	23·9	S. 11·59	S. 5· 8	971
59	1862 Jan. 2	P	110	2·0	1·4	S. 17·50	N. 2·19	967
60	9	P		9·0	8·4	N. 15·56	N. 4·45	889
61	16	P		16·0	15·4	N. 19·23	S. 1·50	918
62	23	P		23·0	22·4	S. 15·37	S. 4·44	967
63	28	S	111	0·7	27·4	S. 22·29	N. 0·34	971
64	Feb. 21	P		24·7	21·9	S. 21·55	S. 3· 9	971
65	23	P		26·7	23·9	S. 24· 7	S. 0·53	965
66	Mar. 1	P	112	5·3	0·3	S. 1·58	N. 4·47	924
67	6	S		10·3	5·3	N. 19·59	N. 3·36	889
68	10	P		14·3	9·3	N. 23·13	S. 0·12	900
69	23	P	113	0·0	22·3	S· 23· 5	N. 0·20	963
70	Oct. 10	P	120	10·4	16·1	N. 18· 3	N. 3· 3	901
71	Nov. 30	P	122	6·8	8·2	N. 3·29	N. 5·12	925
72	Dec. 18	P		24·8	26·2	S. 18· 3	S. 3· 3	981
73	1863 Jan. 10	P	123	20·4	19·8	N. 1·44	S. 5·12	912
74	31	P	124	14·1	11·3	N. 22·18	S. 1· 8	885
75	Feb. 9	P		23·1	20·3	S. 11·19	S. 4· 7	937
76	18	P	125	4·8	29·3	S. 7·56	N. 4·50	980
77	Mar. 12	S		26·8	21·9	S. 22· 2	N. 0· 2	965
78	Apr. 19	P	127	10·1	0·9	N. 16·47	N. 2·29	918
79	May 12	P	128	5·9	23·9	S. 2· 1	N. 5·14	949
80	Nov. 20	S	135	7·2	8·7	N. 1· 4	N. 4·51	956
81	25	P		12·2	13·7	N. 19·42	S. 0· 0	918
82	Dec. 6	P		23·2	24·7	S. 8· 8	S. 3·48	922
83	1864 Jan. 10	P	137	3·5	0·7	S. 16· 1	N. 4·31	1004
84	18	S		11·5	8·7	N. 18·32	N. 0·24	918
85	29	S		22·5	19·7	S. 5· 7	S. 3·53	898
86	Feb. 25	P	138	22·2	17·2	S. 3·56	S. 3·52	895
87	28	P		25·2	20·2	S. 15·17	S. 1· 9	919
88	Apr. 13	P	140	15·6	6·4	N. 18·42	S. 4·13	900
89	22	P		24·6	15·4	S. 13·26	S. 1·32	921
90	1865 Jan. 10	P	150	15·0	12·1	N. 19·33	S. 3·52	928
91	21	P		26·0	23·1	S. 14·38	N. 0·31	906

TABLE I.—*continued.*

Reference No.	Date of Commencement of Cyclone.	Character.	No. of Rotation.	Day of Rotation.	Age of Moon.	Dec. of Moon.	Lat. of Moon.	Semi-diameter of Moon.
				d	d	° ′	° ′	″
92	1865 Feb. 12	S	151	20·7	15·6	N. 4·49	S. 4· 7	888
93	19	P	152	0·3	22·6	S. 18·1	N. 2·31	915
94	Mar. 23	P	153	5·0	25·2	S. 14·17	N. 5·13	969
95	Apr. 14	P		27·0	17·8	S. 16·53	N. 2·15	900
96	Dec. 8	P	162	19·8	19·5	N. 9·48	S. 4·15	921
97	1866 Jan. 6	P	163	21·4	18·8	N. 3·58	S. 2·44	910
98	Feb. 2	P	164	21·1	16·1	N. 5·37	S. 2·54	916
99	Mar. 3	P	165	22·8	15·6	S. 0·48	S. 1· 9	900
100	30	P	166	22·4	13·1	N. 0·18	S. 1·28	898
101	Apr. 6	P	167	2·1	20·1	S. 18· 9	N. 4·54	893
102	13	P		9·1	27·1	S. 1·51	N. 1·53	982
103	Nov. 12	P	175	4·2	4·6	S. 18· 5	N. 5· 5	889
104	30	S		22·2	22·6	N. 3·46	S. 1· 6	921
105	1867 Jan. 15	P	177	13·6	9·0	N. 11·46	S. 3·39	971
106	Feb. 1	P	178	3·2	26·0	S. 18·20	N. 5· 7	889
107	14	P		16·2	9·2	N. 17·55	S. 5·13	976
108	Apr. 11	S	180	17·6	6·1	N. 17·58	S. 4·56	971
109	May 24	P	182	6·1	19·7	S. 16· 5	N. 3·47	891

TABLE II.

Cyclones in the Bay of Bengal.

Reference No.	Date of Commencement of Cyclone.	Character.	No. of Rotation.	Day of Rotation.	Age of Moon.	Dec. of Moon.	Lat. of Moon.	Semi-diameter of Moon.
				d	d	° ′	° ′	″
1	1856 Nov. 17	C	41	12·1	19·1	N. 25·54	N. 5· 5	936
2	1857 June 18	C	49	6·7	25·4	N. 14·39	N. 2·31	985
3	Aug. 9	F	51	4·3	18·7	S. 1· 8	S. 0· 3	962
4	18	F		13·3	27·7	N. 23·22	N. 4·11	951
5	Nov. 2	C	54	7·5	15·1	N. 17·48	N. 3·29	1004
6	1858 Apr. 9	C	60	1·6	24·5	S. 18·33	S. 2·42	917
7	May 15	M	61	10·3	1·7	N. 27·54	N. 5· 2	1000
8	July 23	F	63	24·9	12·1	S. 28·20	S. 4·55	885

TABLE II.—*continued.*

Reference No.	Date of Commencement of Cyclone.	Character.	No. of Rotation.	Day of Rotation.	Age of Moon.	Dec. of Moon.	Lat. of Moon.	Semi-diameter of Moon.
				d	d	° ′	° ′	″
9	1859 Apr. 21	M	73	24·0	17·6	S. 27·18	S. 4·51	908
10	June 12	M	75	21·5	10·7	S. 19·44	S. 4·58	923
11	July 11	C	76	23·3	10·4	S. 25·46	S. 5· 5	907
12	Aug. 25	F	78	13·9	26·1	N. 25·27	N. 2·59	989
13	1860 Apr. 6	C	86	20·4	14·4	S. 11·15	S. 4·43	992
14	July 25	F	90	21·5	6·4	S. 16· 6	S. 5·17	964
15	1861 Aug. 17	F	105	0·5	10·5	S. 23· 5	S. 0· 8	947
16	1862 Aug. 9	F	118	2·9	13·1	S. 16· 6	N. 3·17	977
17	1863 June 16	C	129	13·7	29·3	N. 21·53	S. 1·16	889
18	Aug. 10	F	131	14·3	25·0	N. 21·29	S. 1·56	888
19	28	F	132	5·0	13·4	S. 9·32	N. 4·56	996
20	1864 Oct. 2	H	146	24·2	1·1	S. 9·38	S. 1·41	895
21	25	C	147	19·9	24·1	N. 6·59	S. 4·58	887
22	Nov. 5	C	148	3·6	5·4	S. 17·29	N. 4·54	950
23	1865 Aug. 16	F	158	14·9	24·2	N. 18·49	S. 4·16	945
24	1867 Sept. 30	C	186	26·2	2·0	S. 8·18	N. 3·50	928
25	Oct. 27	C	187	25·9	29·0	S. 7· 0	N. 3·31	927
26	Nov. 12	C	188	14·6	15·5	N. 12·54	S. 4·38	977

TABLE III.

Age of Moon.	No. of Cyclones. Mauritius.	Bengal.	Age of Moon.	No. of Cyclones. Mauritius.	Bengal.	Age of Moon.	No. of Cyclones. Mauritius.	Bengal.
d d			d d			d d		
0– 1	5	...	10–11	1	3	20–21	4	...
1– 2	6	2	11–12	1	...	21–22	4	...
2– 3	4	1	12–13	2	1	22–23	4	..
3– 4	...	...	13–14	5	2	23–24	5	...
4– 5	3	...	14–15	2	1	24–25	6	3
5– 6	2	1	15–16	6	2	25–26	2	2
6– 7	6	1	16–17	4	...	26–27	5	1
7– 8	...	...	17–18	6	1	27–28	2	1
8– 9	5	...	18–19	4	1	28–29	1	...
9–10	7	...	19–20	6	1	29–30	1	2

TABLE IV.

Date of Commencement of Cyclone.		No. of Rotation.	Day of Rotation.
	SEQUENCE No. 1.		
			d
1856	Nov. 7	41	2·1
1857	Jan. 28	44	2·1
	Mar. 23	46	1·4
	SEQUENCE No. 2.		
1857	**June 18**	**49**	**6·7B**
	Nov. 2	**54**	**7·5B**
	Dec. 26	56	6·9
1858	Mar. 18	59	6·9
	SEQUENCE No. 3.		
1857	**Aug. 18**	**51**	**13·3B**
	Dec. 4	55	12·2
1858	Feb. 24	58	12·2
	SEQUENCE No. 4.		
1858	Jan. 12	56	23·9
	Mar. 6	58	22·2
	Apr. 2	59	21·9
	SEQUENCE No. 5.		
1858	Apr. 18	60	10·6
	May 15	**61**	**10·3B**
	SEQUENCE No. 6.		
1858	Dec. 15	69	6·3
1859	Mar. 7	72	6·3
	Apr. 5	73	8·0
	SEQUENCE No. 7.		
1859	Jan. 1	69	23·3
	Apr. 21	73	24·0
	July 11	**76**	**23·3B**
	SEQUENCE No. 8.		
1859	Jan. 24	70	19·0
	Apr. 17	73	20·0
	June 12	**75**	**21·5**
	SEQUENCE No. 9.		
1859	Feb. 16	71	14·7
	Mar. 17	72	16·3
	SEQUENCE No. 10.		
1859	June 3	75	12·5
	Aug. 25	**78**	**13·9B**
	Nov. 16	81	15·1
1860	Jan. 10	83	15·4
	SEQUENCE No. 11.		
1859	Dec. 1	82	2·8
1860	Jan. 25	84	3·1
	Feb. 22	85	3·8
	SEQUENCE No. 12.		
1859	Dec. 9	82	10·8
1860	Feb. 28	85	9·8
	SEQUENCE No. 13.		
1860	Feb. 11	84	20·1
	Apr. 6	**86**	**20·4B**
	May 27	88	18·9
	July 25	**90**	**21·5B**
	SEQUENCE No. 14.		
1860	Mar. 2	85	12·8
	Apr. 26	87	13·1
	SEQUENCE No. 15.		
1860	Dec. 4	95	17·2
1861	Jan. 29	97	18·5
	SEQUENCE No. 16.		
1861	Jan. 18	97	7·5
	Mar. 12	99	5·9
	SEQUENCE No. 17.		
1861	Feb. 28	98	21·2
	Mar. 28	99	21·9

TABLE IV.—*continued.*

Date of Commencement of Cyclone.			No. of Rotation.	Day of Rotation.
		SEQUENCE NO. 18.		d
1861	**Aug.**	**17**	**104**	**27·8B**
1862	Jan.	2	109	29·3
		28	110	28·0
	Feb.	23	111	26·7
	Mar.	23	112	27·3
		SEQUENCE NO. 19.		
1861	Dec.	18	109	14·3
1862	Jan.	16	110	16·0
	Mar.	10	112	14·3
		SEQUENCE NO. 20.		
1861	Dec.	26	109	22·3
1862	Jan.	23	110	23·0
	Feb.	21	111	24·7
		SEQUENCE NO. 21.		
1862	Jan.	9	110	9·0
	Mar.	6	112	10·3
		SEQUENCE NO. 22.		
1862	Nov.	30	122	6·8
1863	Feb.	18	125	4·8
	May	12	128	5·9
	Aug.	**28**	**132**	**5·0B**
	Nov.	20	135	7·2
		SEQUENCE NO. 23.		
1862	Dec.	18	122	24·8
1863	Feb.	9	124	23·1
		SEQUENCE NO. 24.		
1863	Jan.	31	124	14·1
	June	**16**	**129**	**13·7B**
	Aug.	**10**	**131**	**14·3B**
	Nov.	25	135	12·2
		SEQUENCE NO. 25.		
1863	Dec.	6	135	23·2
1864	Jan.	29	137	22·5
	Feb.	25	138	22·2
		SEQUENCE NO. 26.		
1864	Feb.	28	138	25·2
	Apr.	22	140	24·6
		SEQUENCE NO. 27.		
1864	**Oct.**	**25**	**147**	**19·9B**
1865	Feb.	12	151	20·7
		SEQUENCE NO. 28.		
1864	**Oct.**	**2**	**146**	**24·2B**
1865	Jan.	21	150	26·0
	Feb.	19	151	27·6
	Apr.	14	153	27·0
		SEQUENCE NO. 29.		
1865	Dec.	8	162	19·8
1866	Jan.	6	163	21·4
	Feb.	2	164	21·1
	Mar.	3	165	22·8
		30	166	22·4
		SEQUENCE NO. 30.		
1866	Nov.	12	175	4·2
1867	Feb.	1	178	3·2
		SEQUENCE NO. 31.		
1867	Feb.	14	178	16·2
	Apr.	11	180	17·6
		SEQUENCE NO. 32.		
1867	**Sept.**	**30**	**186**	**26·2B**
	Oct.	**27**	**187**	**25·9B**

93 cyclones out of a total of 135 are included in the above sequences.

Cyclones in the Bay of Bengal are printed in heavy type and are marked with a B.

Solar Parallax Papers. No. 8.

The Mass of the Moon, derived from the photographic observations of Eros made in 1900–01. By Arthur R. Hinks, M.A. (Plate 6.)

§ 1. A determination of the mass of the Moon is a natural sequel to an extensive series of observations for the solar parallax. In the present paper I discuss, with this intent, the right ascensions of Eros observed from the beginning of October 1900 to the end of February 1901. A great part of this material has already been discussed for the determination of the solar parallax (S.P.P. No. 7, *M.N.*, 1909 May). For the parallax determination the observations made after Christmas 1900 had very little weight. But, on the other hand, it will appear that the part of the series most valuable for the determination of the mass of the Moon is the latter half, from the middle of December to the end of February. It is very fortunate, therefore, that some observers persevered throughout the unfavourable months of January and February 1901, after those whose main interest was the solar parallax had stopped work.

§ 2. The determination of the mass of the Moon involves some delicate considerations which hardly enter into the determination of the parallax—for the lunar equation has a period of a month, the parallactic displacement of only a day; and small systematic errors of star places or ephemeris, gradually changing, become seriously involved with the former, though they may have scarcely any effect upon the observation of the latter. In the case of Eros there is particular reason to fear for the star places, because the planet described a loop which took it up to 54° N. declination, north of the zenith of most observatories. This gave ample opportunity for the entry of small systematic errors in the meridian observations, depending upon the declination and the direction of motion in the field of the meridian circle.

§ 3. *The Star Systems.*—In previous papers of this series I have described the construction of my photographic star system, based largely upon the photographic star places published in the Paris Circulars, and resting ultimately upon Loewy's discussion of the meridian observations of fundamental stars. The adoption of this system has been adversely criticised by Dr. Fritz Cohn, who has constructed an alternative system founded upon observations made with the Koenigsberg meridian circle, fitted with the Repsold transit micrometer. In S.P.P. No. 4 (*M.N.*, 1906 June, lxvi. No. 8) I gave reasons why it did not seem to be advantageous to undertake the labour of reducing the whole series of photographic observations to Dr. Cohn's system. The questions then raised have been fully discussed between us in the *Astronomische Nachrichten* (Nos. 4119, 4157, 4167), and there are still points of difference outstanding. It seems to me that these

can have no bearing upon my recent parallax work; but it is different with the mass of the Moon. Loewy's system of *repère* stars may very well have a small oscillating discordance from the fundamental system, due to the somewhat varying mean magnitude of the stars at different parts of the planet's path; and it is pretty certain to have a further discordance varying with the declination. I have therefore been very glad to avail myself of Dr. Cohn's Fundamental System for an independent determination of the correction to the assumed lunar equation and mass of the Moon.

§ 4. *Comparison between the Cambridge and Cohn Systems.*—My standard catalogue of photographic star places contains all the *étoiles de repère*; the deduced photographic places may be compared directly with the places of Dr. Cohn's system. My method was to take contiguous areas along the track of the planet, each roughly of the area covered by a plate 2° square; and to take the mean difference "Cambridge – Cohn" for each area. The process was then repeated with areas centred on intermediate points.

The result of this comparison is given in Table I.

Table I.

Comparison between the Cambridge Standard System and Cohn's System.

Date.	No. of Stars.	Cambridge minus Cohn.	Date.	No. of Stars.	Cambridge minus Cohn.
1900.		s	1900–1.		s
Sept. 26·5	24	+·061	Nov. 27·0	21	+·060
29·5	16	·057	29·0	16	·069
Oct. 2·5	21	·057	Dec. 2·0	27	·060
5·5	14	·058	5·5	19	·051
8·5	26	·058	9·5	21	·048
13·0	25	·063	13·5	22	·051
15·0	23	·069	17·0	26	·052
16·5	21	·075	20·0	23	·054
20·5	23	·072	24·0	30	·052
24·0	25	·066	25·5	23	·051
28·0	21	·059	31·5	35	·048
31·0	21	·048	Jan. 7·5	31	·040
Nov. 3·5	20	·038	11·0	25	·037
5·0	22	·045	14·0	29	·034
6·0	26	·044	20·5	24	·037
9·5	19	·045	28·0	44	·033
12·5	22	·044	Feb. 5·0	50	·036
16·0	26	·043	13·0	36	·035
22·5	15	·040	19·5	27	·043
25·0	21	·051	26·0	76	·040

The mean difference, about $+0^{s}.05$, corresponds nearly with the magnitude equation between the fundamental stars and the average *étoile de repère* for the average meridian observer, as it should. But the variations from this mean difference appear to be decidedly greater than the variation which should arise from fluctuations in the mean magnitude of the *étoiles de repère*. How to account for this is not clear. I think, however, that if we plot the quantities in the column Cambridge – Cohn, draw a sinuous curve through them, take out the reduction from Cambridge to Cohn's system for each day, and apply it to the values of C – O, we shall obtain a result which is practically equivalent to a complete reduction to Cohn's system.

It is evident that the fluctuations in the value of Cambridge minus Cohn will not be without influence upon the deduction of the mass of the Moon, and the determination will be much strengthened by this utilisation of a partly independent star system to which special care has been given by its author.

§ 5. *The Ephemerides of the Planet.*—An account of the successive ephemerides furnished by the Paris Observatory has been given in S.P.P. No. 7, paragraphs 8 and 9. The lunar equation was computed at the Bureau des Longitudes by the rigorous formulæ given by Newcomb (Tables of the Sun: *Papers of the American Ephemeris*, vol. vi. p. 18); that is to say, it was computed with Tabular values of the places of the Sun and Moon, and Tabular values of the lunar parallax. It is rigorous, and does not depend upon developments in series. By the kindness of M. Baillaud and M. Lagarde, the terms of the lunar equation in right ascension and declination were exhibited separately in the Paris Ephemeris completed in 1909 February, and in this respect the ephemeris leaves nothing to be desired.

It was, however, still doubtful whether Newcomb's Solar Tables could give the ephemeris of the Earth with the precision required for the determination of the mass of the Moon; it is preferable that the motion of the Earth–Moon should be computed by special perturbations. By the direction of M. Baillaud, M. Lagarde undertook this further heavy work, and in May last I received from Paris the following table:—

Table II.

Corrections to the New Paris Ephemeris of Eros to take account of the New Computation of the Solar Ephemeris by Special Perturbations (Lagarde).

M.T. Berlin.	Δα.	M.T. Berlin.	Δα.	M.T. Berlin.	Δα.
1900.	s	1900.	s	1901.	s
Sept. 23·5	+0·0002	Nov. 16·5	−0·0092	Jan. 7·5	−0·0048
25·5	13	18·5	102	9·5	45
27·5	20	20·5	99	11·5	45
29·5	26	22·5	110	13·5	50
Oct. 1·5	31	24·5	141	15·5	48
3·5	39	26·5	165	17·5	37
5·5	52	28·5	167	19·5	20
7·5	62	30·5	162	21·5	18
9·5	57	Dec. 2·5	153	23·5	15
11·5	51	4·5	160	25·5	12
13·5	55	6·5	173	27·5	− 5
15·5	58	8·5	166	29·5	+ 4
17·5	60	10·5	154	31·5	12
19·5	61	12·5	150	Feb. 2·5	12
21·5	53	14·5	144	4·5	+ 9
23·5	43	16·5	146	6·5	− 1
25·5	33	18·5	130	8·5	10
27·5	18	20·5	110	10·5	10
29·5	+ 6	22·5	95	12·5	6
31·5	− 2	24·5	88	14·5	− 3
Nov. 2·5	15	26·5	77	16·5	0
4·5	31	28·5	69	18·5	0
6·5	50	30·5	66	20·5	− 3
8·5	49	1901.		22·5	+ 1
10·5	40	Jan. 1·5	54	24·5	13
12·5	47	3·5	48	26·5	24
14·5	−0·0074	5·5	−0·0045	28·5	+0·0023

It is understood that M. Baillaud proposes to publish a full account of the computation of new Paris Ephemerides We need, then, remark here only that the difference between the Earth–Moon ephemerides computed respectively from Newcomb's tables and by special perturbations is of a kind to have a sensible effect upon the determination of the mass of the Moon.

§ 6. *Form of the Tabular Error.*—In determining the tabular error of an ephemeris by comparison with observation, it has been usual to assume that the tabular error could be expanded in powers of the time, or in powers of the time divided by the tabular distance. A glance at the diagram (Plate 6) will show that for Eros this procedure was impracticable. After trial and consideration, I was unable to find any reasonable function of t, t and Δ, or t, Δ, and δ, which might be supposed to represent the determined values of C − O. It seemed impossible to proceed in the canonical manner

of determining the mass of the Moon, namely, by forming a series of equations of condition, in which the unknowns were the correction to the assumed mass, and the coefficients in the expression for the tabular error; and this impossibility saved me a severe amount of computation.

The only course appeared to be, to determine the tabular error graphically.

§ 7. *Graphical Determination of the Tabular Error.*—This had been done with apparent success in the parallax solution, both at Greenwich and at Cambridge. But for those solutions it was legitimate to assume that the correction to the Moon's mass is insignificant, and to use the normal places of the planet for every day in constructing the curve.

On the other hand, when it becomes a question of searching for a small correction to the lunar equation, one must lay down the tabular error curve by the points—a fortnight apart—when the lunar equation vanishes. This gives very few points for determining a rather complex curve, and it must be admitted that there may be an *a priori* apprehension lest the method may give systematically wrong results at points where the curvature is considerable. The results seem to show that there is very little in this fear.

§ 8. *The normal Values of* C – O.—Inspection of the curve shows at once that the correction to the assumed value of the lunar equation can be only a small fraction of 1 per cent.; hence its effect upon the C – O must be insensible for a couple of days on each side of the vanishing points.

The normal values of C – O at the epochs of zero lunar equation were formed, then, by taking the weighted means of the whole of the individual values of C – O belonging to groups of four or five days centred upon those epochs; that is to say, the normal value of C – O for any group is

$$\Sigma p(\mathrm{C}-\mathrm{O})/\Sigma p\,;$$

and the normal value of the tabular error for that group is

$$\Sigma p(\mathrm{C}-\mathrm{O}-a\mathrm{P})/\Sigma p,$$

where a is the parallax factor for the observation, and the true parallax is $(1+\mathrm{P})\times$ the assumed parallax.

The value of P was taken +·0007 (see S.P.P. No. 7).

§ 9. *Determination of the Tabular Error of Lagarde's Final Ephemeris.*—Six determinations of the tabular error were made. T.E. No. 4 was based upon fortnightly normal groups, formed as above from the daily means which had already been used to form T.E. No. 3, used in the final parallax solutions. The corrections of Table I. were applied to reduce to comparison with Lagarde's final ephemeris.

[T.E No. 5 was not used in these reductions.]

T.E. No. 6 was based upon groups formed from the materials collected for the "1 day" parallax solutions. They differ from the former in two respects: the weight assigned to Lick has been

diminished, and a certain number of discordant results have been rejected. They are all reduced to Lagarde's final ephemeris.

The normal tabular errors thus found were plotted on millimetre paper. A long flexible ruler was bent to pass precisely through each point, and the curve was drawn. The tabular error for the mean epoch of each single-day group was then read from the curve.

T.E. No. 7 A was derived from the same material as No. 6, but the mean $(C - O - aP)$ for each day was first multiplied by the mean value of $\cos\delta$ for that day. This had to be done at some point of the solution; to do it here made the curve less steep, and the graphical process easier and more certain.

The curve was plotted and drawn, and the list of derived tabular errors for each day was made as in No. 6.

T.E. No. 7 B C D was derived from the same material as the preceding, but plotted independently three times in curves B, C, D. This was done in order to see how far accidental errors of plotting and reading from the curve were important. The result shows that they were small, but not quite negligible. T.E. No. 8 A was derived in the same way as No. 7 A, with the exception that the values of C – O were referred to Cohn's Standard System, in the manner described in § 4. T.E. No. 8 B C D corresponds in the same way to No. 7 B C D.

The normal places upon which all these determinations of Tabular Error depend are collected in Table III. which follows. In comparing them, it should be remembered that Nos. 4 and 6 depend upon somewhat different weighting of not quite the same material, and that Nos. 7 and 8 depend upon precisely the same observational material as No. 6.

Table III.

Normal Values of Tabular Error about Epochs when Lunar Equation vanishes.

	Epoch.	T.E. No. 4 C – O.	T.E. No. 6 C – O.	T.E. No. 7 (C – O) $\cos\delta$.	Camb. – Cohn $\cos\delta$.	T.E. No. 8 (C – O + k*) $\cos\delta$.
	1900.	s	s	s	s	s
O.	Sept. 27·8	...	+ ·027	+ ·020	+ ·043	+ ·063
I.	Oct. 9·9	+ ·077	·079	·053	·040	·093
II.	24·1	·154	·158	·097	·040	·137
III.	Nov. 6·8	·215	·216	·126	·026	·152
IV.	19·4	·191	·191	·114	·026	·140
V.	Dec. 1·8	·114	·114	·072	·040	·112
VI.	16·5	·073	·073	·052	·038	·090
VII.	29·8	·060	·058	·045	·038	·083
VIII.	Jan. 14·4	·055	·055	·047	·030	·077
IX.	28·4	·053	·052	·046	·032	·078
X.	Feb. 12·6	·041	·041	·038	·033	·071
XI.	25·8	...	+ ·022	+ ·021	+ ·038	+ ·059

* In the heading of the last column k = Cambridge – Cohn from the preceding column.

§ 10. The adopted tabular errors (in the different series) were read from the curve for the epochs of the daily normal values of C – O. We can judge how far the graphical process is successful in giving consistent results by looking at the deduced tabular errors for epochs midway between the fortnightly points on which the curves were plotted.

Table IV. gives the necessary extracts from the tables of daily tabular error.

Table IV.

Concluded Values of Tabular Error at Intermediate Epochs when Lunar Equation is a Maximum.

		T.E. No. 4 C – O.	T.E. No. 6 C – O.	T.E. 7 A (C – O) cos δ.	T.E. 7 BCD (C – O) cos δ.	T.E. 8 A (C – O + *k*) cos δ.	T.E. 8 BCD (C – O + *k*) cos δ.
		s	s	s	s	s	s
α	Oct. 3·5	...	+ ·048	+ ·034	+ ·035	+ ·073	+ ·077
β	16·5	+ ·113	·116	·075	·073	·115	·113
γ	30·7	·190	·195	·115	·115	·149	·149
δ	Nov. 12·6	·214	·214	·126	·125	·149	·149
ε	26·5	·148	·147	·092	·091	·125	·125
ζ	Dec. 9·4	·088	·087	·060	·061	·101	·099
η	23·4	·066	·066	·046	·048	·086	·086
θ	Jan. 6·5	·056	·053	·044	·046	·080	·080
κ	21·6	·054	·053	·047	·047	·079	·077
λ	Feb. 4·3	·049	·047	·041	·043	·076	·075
μ	19·5	...	·031	·031	·029	·066	·066

The quantities in columns 3 and 4 should be nearly identical; they depend on different weightings of much the same material. Columns 5 and 6 depend on identical material; any difference between them shows the variation which may arise from the graphical processes. The same applies to columns 7 and 8.

§ 11. The daily normal values of O – C were formed on the principle explained in § 8. Actually, they were formed in a perfectly obvious way from the normal equations in $\Delta_0 a$ in the parallax solutions for the single-day groups (S.P.P. No. 7, p. 561, § 20), using the adopted values of P + ·0007.

§ 12. *The Equations of Condition.*—Let the true lunar equation term be $(1 + \mu)$ times the term exhibited by the ephemeris.

Then the true place of Eros = the ephemeris place
+ tabular *correction* due to other causes
+ μ × computed lunar equation,

or μ (computed lunar equation) = tabular *error* + O – C.

Then if a = computed lunar equation expressed in $0^s{\cdot}1$ as unit
n = tabular error $+O-C$,, $0^s{\cdot}001$,,

the working form of equation of condition is

$$100\ \mu.a = n.$$

This is on the assumption that the only unknown which may be introduced is the correction to the lunar equation. But this evidently puts the severest possible strain upon the comparison star places. It is equivalent to the assumption that the system is so smooth that it has practically no sinuosities with half periods shorter than a fortnight. If any such exist, their roughness will appear as an increase in the probable error of mass of the Moon, which will be exhibited thus in the worst light possible.

Solutions I. to VII. had this form of equation of condition, one for each daily normal value of O – C. These were grouped in half sidereal periods of the Moon; that is to say, each group covered the space between one normal point of the tabular error curve and the next. These groups were lettered α, β, γ . . . . μ and the approximate middle date of each is given in Table IV.

§ 13. *Second form of Equation of Condition.*—If we rearrange the material in groups covering a complete lunar sidereal period, we may introduce a constant into the equations of condition, and throw some of the roughness of star and planet places on to a residuary tabular error. Let us call it ΔT. Then the second form of equation of condition is

$$\Delta T + (100\,\mu)a = n.$$

The material collected above in groups α, β, γ . . . . was now rearranged in overlapping monthly groups, whose composition was $\alpha + \frac{1}{2}\beta$, $\frac{1}{2}(\beta+\gamma)$, $\frac{1}{2}(\gamma+\delta)$ . . . . $\frac{1}{2}\lambda+\mu$, and which are designated

$$\alpha\beta, \quad \beta\gamma, \quad \gamma\delta, \ . \ . \ . \ . \ \lambda\mu.$$

Solutions VIII. and IX. depend upon this form.

The resulting values for μ will be given in their proper places. We may anticipate the results of the solutions to say that ΔT reached $^s{\cdot}0005$ in one group only, the first, and was generally much smaller.

§ 14. *The Solutions.*—The brief results of the eight solutions are given in Table V.

The essential differences between the bases of the various solutions are these:—

I. depends on an old system of weights.

III. to IX. on the final system.

I. and III. depend on tabular errors of the form C – O, and the original weights of each group have been multiplied by $\cos^2\delta$; the others depend on tabular errors of the form $(C-O)\cos\delta$, and the lunar equation coefficients have been multiplied correspondingly by $\cos\delta$.

I., III., IV., VI., and VIII. are based on the Cambridge Standard System; V., VII., and IX. on Cohn's System.

I. to VII. are derived from equations of condition in which the correction to the lunar equation is the only unknown; in VIII. and IX. a second unknown, a correction to the tabular error, is introduced.

§ 15. *Discussion of the Eight Solutions.*—It is not easy to decide the relative weights of the eight solutions.

If we take weights proportional to the inverse squares of the probable errors assigned in Table V. we have—

$$0\cdot9 : 0\cdot7 : 0\cdot7 : 2\cdot0 : 1\cdot0 : 1\cdot7 : 2\cdot9 : \text{and } 5\cdot4$$

as the respective weights of the eight solutions, and the combined result—

$$100\,\mu = +\cdot0875 \pm \cdot0679.$$

If we assign a system of weights

$$1 : 1 : 2 : 3 : 4 : 5 : 6 : 8,$$

in accordance with an independent estimate of the probable worth of the different solutions, we have

$$100\,\mu = +\cdot0831 \pm \cdot0722.$$

And if we consider that only solutions VIII. and IX. should be retained, and that IX. should have twice the weight of VIII., in accordance with its better internal agreement, we have

$$100\,\mu = +\cdot085 \pm \cdot048.$$

Evidently any reasonable system of weights gives a result close to $+\cdot085$; and we may adopt this as the definitive value of $100\,\mu$.

But it is not so easy to assign a probable error to the result. The eight solutions are not independent, but are based on the same material. There is no canonical way of deriving the probable error of the result from the separate probable errors of the solutions. We must not do injustice to the later solutions by attaching importance to the larger probable errors of earlier solutions which give the same value for $100\,\mu$. Nor may we adopt what might seem the plausible idea that at any rate the probable error of the mean must be less than the smallest probable error of any contribution to the mean. On consideration, I am inclined to assign to the final result the probable error $\pm\cdot048$, which is the weighted mean of the probable errors of solutions VIII. and IX.

Our definitive result is therefore

$$100\,\mu = +\cdot085 \pm \cdot048.$$

§ 16. *The Mass of the Moon.*—Let E be the ratio of the mass of the Earth to that of the Moon, adopted in the computation of the lunar equation.

ΔE the correction to this value required by the above value of μ.

π the value of the solar parallax adopted in the calculation of the lunar equation.

$\Delta\pi$ the correction to this value given by the discussion of the Eros photographs.

Then $$\frac{\pi+\Delta\pi}{E+\Delta E+1}=\frac{\pi}{E+1}\times(1+\mu),$$

which reduces to

$$\Delta E=\frac{1}{1+\mu}\left[\frac{E+1}{\pi}.\Delta\pi-\mu(E+1)\right].$$

Now $E=81{\cdot}45$ (Newcomb's value).

$\pi=8''{\cdot}790$ (the value used in Newcomb's Tables of the Sun).

$\Delta\pi=+''{\cdot}016\pm{\cdot}0026$ (S.P.P. No. 7).

$$\text{Hence } \Delta E=\frac{1}{1{\cdot}00085}\left\{\begin{array}{l}9{\cdot}38(+{\cdot}016\pm{\cdot}0026)\\ \qquad -82{\cdot}45(+{\cdot}00085\pm{\cdot}00048)\end{array}\right\}$$

$=+0{\cdot}080\pm{\cdot}047$, where the probable error includes the uncertainty due to the probable error of the correction to the Solar Parallax.

And our final result is—

Mass of Earth/Mass of Moon $=81{\cdot}53\pm{\cdot}047$.

§ 17. *The Constant of Nutation and the Mechanical Ellipticity of the Earth.*—The relations between these quantities and the mass of the Moon are given by Newcomb (*Astronomical Constants*, p. 132).

$$\text{Constant of Nutation}=\left[5{\cdot}36542\right]\frac{1}{E+1}\cdot\frac{C-A}{C}$$

$$\text{Luni-solar Precession}=\left\{[5{\cdot}93759]\frac{1}{E+1}+[3{\cdot}68762]\right\}\frac{C-A}{C}.$$

And on Newcomb's authority we may regard the numerical coefficients "as absolute constants, so accurately determined that no question of their errors need now be considered." They are reduced to the obliquity for 1850, viz. $23^\circ\ 27'\ 3''{\cdot}7$.

Again, according to Newcomb, the luni-solar precession for 1850 was $50''{\cdot}368$.

Substituting this value in the above equations and solving, we have—

Constant of Nutation $=9''{\cdot}213$.
Mechanical ellipticity of Earth $={\cdot}003278$.

§ 18. It remains to compare these results with previous determinations. In *Cape Annals*, vol. vi., part 6, p. 12, Sir David Gill finds—

$$E=81{\cdot}702\pm{\cdot}094.$$

But he allows me to point out that there is a small error in the deduction of the value of E, and a more considerable one in the deduction of the probable error.

The corrected results are—

$$E = 81\cdot72 \pm \cdot12,$$

or, reduced to $\pi = 8\cdot806$, $E = 81\cdot76$.

In *Astronomical Constants*, page 189, Newcomb gives—

From the Lunar Equation, $E = 81\cdot32 \pm \cdot20$ . . (1)
Observed Constant of Nutation, $E = 81\cdot58 \pm \cdot20$ . . (2)
And adopts as definitive, $E = 81\cdot45 \pm \cdot15$ . . (3)

But, in the light of newer data, these must be revised.

If P is the coefficient of the principal inequality in the lunar equation in longitude, we have, according to Newcomb (p. 189, *loc. cit.*),

$$(1 + E)P = [1\cdot78207]\pi.$$

Newcomb derived his result (1) as follows:—

From the observed longitudes of the Sun, . . $P = 6''\cdot485$
From Gill's observations of Victoria, as published in a preliminary way in *M.N.*, liv. 350, ... $P = 6\ \cdot446$
Mean . $P = 6\ \cdot465$

which with $\pi = 8\cdot790$ gave . . . $E + 1 = 82\cdot32$, as above,
but with $\pi = 8\cdot806$ gives . . . $E + 1 = 82\cdot47$,
and if we use Gill's final result, $P = 6''\cdot443$,
instead of his preliminary, we have . $E + 1 = 82\cdot48$.

Newcomb's result (2) was derived from the observed nutation constant $9''\cdot21$, and the assumed luni-solar precession $50''\cdot36$.

But if we use his later value, $50''\cdot368$, for the latter quantity, we obtain . . . . . $E + 1 = 82\cdot62$.

Hence the revised values, according to Newcomb's data, are—

From the Lunar Equation, . . . $E = 81\cdot48$.
From the Constant of Nutation, . $E = 81\cdot62$.
Mean . $E = 81\cdot55$ instead of $81\cdot45$.

Retaining the probable error assigned by Newcomb, we have thus, when all three results are referred to $\pi = 8''\cdot806$,

Newcomb (revised), ... $E = 81\cdot55 \pm \cdot15$
Gill (corrected), ... $E = 81\cdot76 \pm \cdot12$

to compare with

Cambridge, ... $E = 81\cdot53 \pm \cdot047$

And, without laying stress upon the probable errors, we may conclude that the observations of Eros have made a considerable reduction in the uncertainty attached to our knowledge of the mass of the Moon.

§ 19. *A neglected Aberration Term.*—After the above solutions had been completed, Professor Turner very kindly called my

attention to the small aberration produced by the motion of the Earth about the centre of gravity of the Earth and Moon, and inquired whether I had taken it into account. I had not. But, after some anxiety, I have concluded that it can produce no sensible effect in the determination of the mass of the Moon.

The argument is as follows:—

Together with the ordinary circular and the diurnal aberration, it is taken out in the usual process of reducing the photograph of the planet direct to mean place.

When the planet is brought up again to apparent place, to compare with the antedated ephemeris, only the circular aberration is added.

But the ephemeris which is antedated is a true geocentric ephemeris, and contains the lunar equation.

Hence the process of antedating allows for the circular aberration + the lunar aberration, whereas only the circular aberration is included in the apparent observed place of the planet. In other words, antedating does too much: it eliminates lunar aberration again, when it has already been taken away once automatically. Hence the final value of O – C involves the lunar term.

Its maximum value, however, is only about $0''\cdot01$, and the maximum occurs at the epochs when the lunar equation vanishes.

The normal places from which the "lunar-equation-free" tabular error was derived are not "lunar-aberration-free." The tabular error has a periodic term with a semi-amplitude of about $''\cdot01$ and period a sidereal month; but the phase differs a quarter period from the periodic term due to the lunar equation, of which we have been in search. And it is impossible to suppose that the star places are smooth and free from systematic error within several hundredths of a second. I conclude that the lunar aberration term has produced no sensible effect on our results.

But it may be interesting to inquire how to deal strictly with this small difficulty on a future occasion. It appears to me that the ephemeris of the planet should not be geocentric, but referred to the centre of gravity of the Earth and Moon; and the lunar equation should be given separately and explicitly.

The observed place of the planet should then be corrected for lunar equation as it is corrected for diurnal parallax—reduced; that is to say, not to geocentric but to geo-selenocentric place, if if we may coin a word. The process of antedating in the geo-selenocentric ephemeris then allows for the circular aberration of light + the eccentricity term which is inherent in the star places; and the final place of the planet is free from all aberrations.

§ 19. As before, I have to acknowledge gratefully the assistance in computation provided by the grant from the Government Grant Fund of the Royal Society.

Cambridge:
1909 *November* 10.

Mean Areas and Heliographic Latitudes of Sun-spots in the Year 1906, deduced from Photographs taken at the Royal Observatory, Greenwich; at Dehra Dûn and at Kodaikánal Observatory, India; and in Mauritius.

(*Communicated by the Astronomer Royal.*)

The results here given are in continuation of those printed in the *Monthly Notices*, vol. lxvii. p. 198, and are deduced from the measurements of photographs taken at the Royal Observatory, Greenwich; at Dehra Dûn; at the Kodaikánal Observatory, India; and at the Royal Alfred Observatory, Mauritius.

The great delay which has taken place in the preparation of these results has been due to a disturbance of the adjustment of the Dehra Dûn photoheliograph. This was discovered when the measures and reductions of the photographs for 1906 were completed, and the results for different days were collated in the preparation of the Ledger of Groups of Sun-Spots, the latitudes as given by the photographs taken at Dehra Dûn showing considerable discordances, as compared with each other and with the latitudes derived from the Greenwich and Kodaikánal photographs. It became necessary, therefore, to reject 147 photographs taken at Dehra Dûn that had been already measured and reduced for the year 1906, and to substitute for them photographs taken at Kodaikánal or Mauritius. A large number of Dehra Dûn negatives for 1907 had also been already measured, and had similarly to be rejected; and photographs from Kodaikánal or Mauritius were obtained in their stead, and also to fill up the gaps in the series obtained at Greenwich up to 1908 February 20, when the Dehra Dûn photoheliograph was again put into proper adjustment. This work of remeasurement has now been completed.

The following results are given in the same form as in preceding communications. Table I. gives the mean daily area of umbræ, whole spots and faculæ for each synodic rotation of the Sun in 1906; and Table II. gives the same particulars for the entire year 1906 and the five preceding years, for the sake of comparison. The areas are given in two forms: first, projected areas; that is to say, as seen and measured on the photographs, these being expressed as millionths of the Sun's apparent disc; and next, areas as corrected for foreshortening, the areas in this case being expressed in millionths of the Sun's visible hemisphere.

Table III. exhibits for each rotation in 1906 the mean daily area of the whole spots (corrected for foreshortening), and the mean heliographic latitude of the spotted area for spots north and for spots south of the equator, together with the mean heliographic latitude of the entire spotted area and the mean distance from the equator of all spots; and Table IV. gives the same information for the year as a whole, similar results for the four preceding years being added as in the case of Table II.

Tables II. and IV. are thus in continuation of the similar tables for the years 1874 to 1888 on pp. 381 and 382 of vol. xlix. of the *Monthly Notices*, and for the years 1889 to 1902 on pp. 465 and 466 of vol. lxiii., and for the years 1901 to 1905 on pp. 199 and 200 of vol. lxvii.

The rotations in Table I. and Table III. are numbered in continuation of Carrington's series (*Observations of Solar Spots made at Redhill*, by R. C. Carrington, F.R.S.), No. I. being the rotation commencing 1853 November 9. The assumed prime meridian is that which passed through the ascending node at mean noon of 1854 January 1, and the assumed period of the Sun's sidereal rotation is 25·38 days. The dates of the commencement of the rotations are given in Greenwich civil time, reckoning from mean midnight.

TABLE I.

No. of Rotation.	Date of Commencement of each Rotation.	No. of Days on which Photographs were taken.	Mean of Daily Areas.					
			Projected.			Corrected for Foreshortening.		
			Umbræ.	Whole Spots.	Faculæ.	Umbræ.	Whole Spots.	Faculæ.
	1905. d							
699	Dec. 26·08	28	118	743	2503	88	562	2576
	1906.							
700	Jan. 22·42	27	138	958	2377	97	697	2554
701	Feb. 18·77	27	101	677	2307	74	521	2424
702	Mar. 18·09	27	281	1853	2475	193	1319	2622
703	Apr. 14·38	28	96	689	2426	74	547	2641
704	May 11·62	27	226	1570	2354	171	1212	2570
705	June 7·83	27	218	1432	2636	147	978	2629
706	July 5·02	27	296	1813	2884	213	1320	3003
707	Aug. 1·23	27	126	836	2285	94	642	2298
708	28·47	28	152	1009	1929	102	716	2010
709	Sept. 24·73	27	18	144	1434	15	136	1502
710	Oct. 22·02	28	67	467	1094	45	320	1197
711	Nov. 18·33	27	126	835	1992	101	697	2089

TABLE II.

Year.	No. of Days on which Photographs were taken.	Mean of Daily Areas.					
		Projected.			Corrected for Foreshortening.		
		Umbræ.	Whole Spots.	Faculæ.	Umbræ.	Whole Spots.	Faculæ.
1901	359	14	41	23	9	29	29
1902	349	14	86	163	10	62	178
1903	350	67	434	875	51	340	970
1904	363	93	653	1639	67	488	1761
1905	364	230	1637	2433	163	1191	2612
1906	364	158	1047	2207	114	778	2320

TABLE III.

No. of Rotation.	Date of Commencement of each Rotation.	No. of Days on which Photographs were taken.	Spots NORTH of the Equator. Mean of Daily Areas.	Spots NORTH of the Equator. Mean Heliographic Latitude.	Spots SOUTH of the Equator. Mean of Daily Areas.	Spots SOUTH of the Equator. Mean Heliographic Latitude.	Mean Heliographic Latitude of entire Spotted Area.	Mean Distance from Equator of all Spots.
	1905. d							
699	Dec. 26·08	28	295	10°·94	267	9°·02	+1°·46	10°·03
	1906.							
700	Jan. 22·42	27	425	14·92	272	10·94	+ 4·83	13·37
701	Feb. 18·77	27	387	13·21	134	10·54	+ 7·11	12·57
702	Mar. 18·09	27	905	16·68	414	13·54	+ 7·19	15·70
703	Apr. 14·38	28	405	14·22	142	21·03	+ 5·09	15·98
704	May 11·62	27	1068	11·89	144	19·21	+ 8·19	12·76
705	June 7·83	27	803	14·96	175	10·84	+10·34	14·22
706	July 5·02	27	1080	14·26	240	13·26	+ 9·26	14·08
707	Aug. 1·23	27	273	19·35	369	14·31	− 0·01	16·45
708	28·47	28	412	10·88	304	12·78	+ 0·84	11·69
709	Sept. 24·73	27	16	16·35	119	18·34	−14·14	18·10
710	Oct. 22·02	28	92	13·70	228	11·70	− 4·40	12·28
711	Nov. 18·33	27	473	14·16	225	16·68	+ 4·22	14·98

TABLE IV.

Year.	No. of Days on which Photographs were taken.	Spots NORTH of the Equator. Mean of Daily Areas.	Spots NORTH of the Equator. Mean Heliographic Latitude.	Spots SOUTH of the Equator. Mean of Daily Areas.	Spots SOUTH of the Equator. Mean Heliographic Latitude.	Mean Heliographic Latitude of entire Spotted Area.	Mean Distance from Equator of all Spots.
1901	359	22	8°·59	7	16°·27	+2°·82	10°·37
1902	349	42	18·81	21	15·29	+7·48	17·64
1903	350	132	18·11	208	21·15	−5·85	19·94
1904	363	268	16·33	220	16·88	+1·37	16·57
1905	364	750	11·66	440	15·55	+1·60	13·10
1906	364	539	13·98	239	14·01	+5·38	13·99

The principal features of the record for 1906 are—

1. A heavy falling off in the mean daily spotted area as compared with 1905; the umbræ showing a decrease of 30 per cent., the whole spots of 35 per cent.

2. This decrease has not been regular throughout the year,

three rotations having exceeded the average of 1905. On the other hand, five rotations in 1905 exceeded in area the most active of 1906. The decrease still left the level of 1906 very considerably above that of 1904.

3. The faculæ have, as usual, been much less subject to variations in area than the spots; and though they have partaken of the decline shown by the spots, the falling off as compared with 1905 has only amounted to 11 per cent. The faculæ, however, have varied more than in 1905; and after remaining pretty steady in area until July, showed a sharp decline during the rest of the year.

4. Comparing the whole spots of the two hemispheres, the northern hemisphere has still further increased its superiority over the southern, the area for the former being to that of the latter as 69 to 31. This increase was still more marked during the first seven months of the year, for which the proportion was as 71 to 29. The spotted areas of the two hemispheres were practically equal during the last five months. In the two preceding cycles the balance has been turned heavily in favour of the southern hemisphere by the time that the maximum has been reached.

5. The mean distance from the equator of all spots has not differed much from the value shown in 1905, but has exhibited a slight upward movement. The mean value, 14°, does not differ greatly from those obtained in the two last years of maximum, 13°·06 in 1883, and 14°·49 in 1893.

6. The Sun was free from spots on five days in 1906, as against two days in 1905.

7. The distribution of spots in latitude has suffered little change from 1905, every latitude from the equator up to 31° being represented, an arrangement usually characteristic of the maximum years of the cycle.

8. The number of separate groups of spots was 351, as compared with 353 in the previous year, or practically the same number. The average size, therefore, had diminished by fully one-third. This was due to the absence of the abnormally large groups which were so conspicuous in the record of 1905.

9. Of the 351 separate groups, 215 were in the northern hemisphere and 136 in the southern.

Royal Observatory, Greenwich:
1909 *November* 9.

Aberration Day Numbers for 1910. By H. C. Plummer, M.A.

In the notation adopted in the *Nautical Almanac* the effects of aberration are given (in arc) by

$$\Delta\alpha \cos\delta = \mathrm{A}\cos\alpha + \mathrm{B}\sin\alpha$$
$$\Delta\delta \quad = i\cos\delta - \mathrm{A}\sin\alpha\sin\delta + \mathrm{B}\cos\alpha\sin\delta$$

where

$$\mathrm{A} = -20''{\cdot}47\cos\omega\cos\odot, \quad \mathrm{B} = -20''{\cdot}47\sin\odot, \quad i = \mathrm{A}\tan\omega$$

The latter formulæ are not exact, chiefly for the reason that no account is taken of the effect of the Moon on the Earth's motion. In view of this fact other formulæ were given (in *Monthly Notices*, lxix. p. 414), by which it was hoped that the aberration could be calculated with no systematic or periodic error exceeding $0''{\cdot}001$. These formulæ may be written—

$$\mathrm{A} = -[3{\cdot}37650](\Delta\mathrm{Y} - \tfrac{1}{2}\Delta^2\mathrm{Y} + \tfrac{1}{3}\Delta^3\mathrm{Y} + b)$$
$$\mathrm{B} = +[3{\cdot}37650](\Delta\mathrm{X} - \tfrac{1}{2}\Delta^2\mathrm{X} + \tfrac{1}{3}\Delta^3\mathrm{X} + a)$$
$$i = -[3{\cdot}37650](\Delta\mathrm{Z} - \tfrac{1}{2}\Delta^2\mathrm{Z} + \tfrac{1}{3}\Delta^3\mathrm{Z} + c)$$

where

$$a = -{\cdot}0001414 - {\cdot}0000036\sin l_{\mathrm{J}} - {\cdot}0000008\sin l_{\mathrm{S}}$$
$$b = -{\cdot}0000257 + {\cdot}0000033\cos l_{\mathrm{J}} + {\cdot}0000007\cos l_{\mathrm{S}}$$
$$c = -{\cdot}0000112 + {\cdot}0000014\cos l_{\mathrm{J}} + {\cdot}0000003\cos l_{\mathrm{S}}.$$

These involve the mechanical differentiation of the 12-hourly solar ephemeris which gives X, Y, Z, while l_{J} and l_{S} are the heliocentric longitudes of Jupiter and Saturn.

In order to test the practical effect of changing the method of calculation, it seemed desirable to have the day numbers computed for a whole year, and to compare them with the numbers given in the *Nautical Almanac*. Professor Turner very kindly agreed to have the calculations (which are very simple and convenient in practice) made, and the results are given at the end of this note. The comparison of $\log\mathrm{A}$, $\log\mathrm{B}$, and $\log i$ with the corresponding numbers in the *Nautical Almanac* can easily be made, but the results are not of direct significance, because when the quantities themselves are small, a considerable difference in the logarithms is of little importance. When the quantities are relatively large the logarithms are sometimes found to differ by 2 or 3 in the fourth place, so that it cannot be said that the numbers in the *Nautical Almanac* possess an accuracy corresponding to the number of decimal places given. The comparison becomes significant throughout when the numbers themselves, and not the logarithms, are compared, and the differences are given accordingly in the unit $0''{\cdot}001$. The run of the differences is not quite even, and this is

in part due to the fact that the 4-figure logarithms of the *Nautical Almanac* can scarcely give the third decimal place of a second of arc with certainty. The deviations from a perfectly regular run, however, do not seem important.

The nature of the differences was tested by grouping them for every 10° of the Moon's longitude and taking means. When the means were arranged in the order of the Moon's longitude, simple inspection showed that they could be represented very approximately by—

$$\Delta A = +0''.0050 - 0''.0075 \cos ☾$$
$$\Delta B = +0''.0005 - 0''.0088 \sin ☾$$
$$\Delta i = +0''.0020 - 0''.0038 \cos (☾ - 10°).$$

These figures were first obtained without calculation, and without knowledge of the magnitudes to be expected. The mean effects during the year of the action of Jupiter and Saturn on the Sun, and those arising from the motion of the Earth about the centre of gravity of the Earth–Moon system were then calculated and found to be with sufficient accuracy—

$$\Delta A = +0''.0049 - 0''.0078 \cos ☾$$
$$\Delta B = +0''.0017 - 0''.0087 \sin ☾$$
$$\Delta i = +0''.0024 - 0''.0039 \cos (☾ - 10°).$$

The latter motion was assumed to be circular, in a plane inclined at $26\frac{1}{2}°$ to the equator, and intersecting this plane in longitude 10°. The general agreement is evident, and shows (1) that the proposed formulæ for the calculation of the effects of aberration are sound, and (2) that the differences from the *Nautical Almanac* are attributable almost entirely to the reactions between the Earth and the Moon, and between Jupiter (and Saturn) and the Sun. The only discrepancy found is a small one between the mean values of ΔB, the source of which has not been ascertained. The effect of the major planets on ΔA and Δi happens to be practically constant throughout the year, while the effect on ΔB, which seems to be partially compensated, steadily increases from the beginning of the year 1910. The new formulæ allow for all perturbations of the Earth's velocity, but, as was to be expected, any effects besides those to which attention has been drawn must be very small.

The fact that the aberration day numbers given in the national ephemerides are not absolutely accurate is certain, and the nature of the chief neglected terms is established. The amount by which the position of a star is affected by the approximate nature of the formulæ of the *Nautical Almanac* (or any other national ephemeris) is not large, but it may be as much as $0''.015$, and it is of a definite, periodic character. We are thus led to consider seriously how the aberration day numbers ought to be calculated. On the one hand,

there are classes of observation which seem to demand more accurate day numbers. Such are observations made expressly for the determination of the constant of aberration, or for the investigation of the variation of latitude, in which the influence of aberration is of capital importance. There are also special problems like a determination of the Moon's mass from refined position observations of a minor planet.

On the other hand, the differences are for many purposes practically negligible. This is probably the case with ordinary meridian observations, although (as Professor Turner has pointed out to me) corrections no greater than 0″·01 are sometimes applied to Greenwich observations of the Moon's N.P.D. There is also the difficulty that the corrections to the day numbers are largely periodic, the period being a lunar month. For this reason they cannot well be applied to a ten-day ephemeris like the clock-star list. The same difficulty applies to a number of small terms (chiefly of fortnightly period) in the lunar nutation, which are accordingly tabulated separately under the headings C′ and D′. Some of the latter terms are comparable in magnitude with the terms under discussion.

The points to be considered seem to be these :—

(1) Whether the effect of the terms in question on the most refined classes of observation is of real moment, or whether they can be safely ignored as hitherto?

(2) Whether the fact that the day numbers are represented by 4-figure logarithms does not implicitly require a corresponding accuracy? Those who oppose the reduction of day numbers to three figures may be supposed to attach importance to the accuracy of the fourth figure.

(3) Whether consistency requires the same treatment for similar terms occurring in the aberration and in the nutation?

(4) If a more accurate calculation of the aberration day numbers be thought desirable, whether they should be calculated directly in a more accurate form and so tabulated, or whether the corrections need be separately tabulated, as is done at present in the case of nutation? The former alternative seems on the whole free from any serious practical objection.

In the following table the first column gives the date, and the next three columns give the values of log A, log B, and log *i* (in the notation of the *Nautical Almanac*) for the corresponding Greenwich mean midnight. The next three columns contain a comparison between the values of A, B, *i* with the *Nautical Almanac*, in the sense of *corrections* to the N.A., and expressed in the unit 0″·001. Finally, in the last column is given the approximate longitude of the Moon, on which it will be seen that the run of the preceding columns is closely dependent.

Aberration Day Numbers for 1910.

	log A.	log B.	log *i*.	ΔA.	ΔB.	Δ*i*.	☾.
Jan. 1	−0·54198	+1·30343	−0·17927	+14	−1	+6	174°
2	0·58119	1·30194	0·21836	13	+2	6	186
3	0·61683	1·30029	0·25404	13	4	6	198
4	0·64983	1·29850	0·28696	11	5	6	209
5	0·68030	1·29658	0·31732	10	4	6	221
6	0·70870	1·29447	0·34582	9	6	5	233
7	0·73518	1·29224	0·37235	8	6	4	245
8	0·76006	1·28982	0·39728	7	5	3	258
9	0·78345	1·28727	0·42057	5	8	3	271
10	0·80554	1·28454	0·44271	4	6	2	284
11	0·82644	1·28164	0·46364	+ 1	7	+1	297
12	0·84619	1·27861	0·48347	0	5	0	311
13	0·86495	1·27537	0·50224	− 1	3	0	325
14	0·88282	1·27198	0·52001	2	3	0	339
15	0·89974	1·26842	0·53703	1	+1	−1	353
16	0·91593	1·26467	0·55320	2	−2	1	7
17	0·93134	1·26077	0·56868	− 1	5	1	21
18	0·94609	1·25672	0·58343	0	3	0	36
19	0·96022	1·25246	0·59763	+ 2	6	−1	50
20	0·97369	1·24809	0·61107	2	8	0	64
21	0·98671	1·24348	0·62403	4	8	+2	77
22	0·99915	1·23872	0·63650	3	11	2	91
23	1·01111	1·23380	0·64850	7	8	2	105
24	1·02259	1·22870	0·65991	10	8	4	118
25	1·03372	1·22338	0·67105	12	8	4	131
26	1·04443	1·21788	0·68177	11	5	5	144
27	1·05473	1·21218	0·69199	12	4	6	157
28	1·06467	1·20627	0·70206	14	1	5	169
29	1·07431	1·20014	0·71162	13	−2	5	181
30	1·08356	1·19381	0·72085	12	0	6	193
31	1·09251	1·18722	0·72978	12	+3	5	205
Feb. 1	1·10118	1·18037	0·73844	12	3	5	217
2	1·10951	1·17328	0·74672	12	6	5	229
3	1·11762	1·16596	0·75477	8	5	5	241
4	1·12539	1·15834	0·76261	6	8	4	253
5	1·13290	1·15046	0·77013	5	8	4	266
6	1·14015	1·14228	0·77740	5	9	3	279
7	1·14718	1·13376	0·78442	+ 1	5	+1	292
8	1·15391	1·12498	0·79122	0	3	0	306
9	−1·16044	+1·11582	−0·79773	− 1	+4	0	320

Aberration Day Numbers—continued.

		log A.	log B.	log *i*.	ΔA.	ΔB.	Δ*i*.	☾.
Feb.	10	−1·16667	+1·10631	−0·80395	−1	+3	−1	334
	11	1·17273	1·09646	0·80997	4	+2	1	349
	12	1·17851	1·08631	0·81582	3	0	2	3
	13	1·18404	1·07564	0·82136	−1	−4	1	18
	14	1·18941	1·06470	0·82672	0	5	0	32
	15	1·19456	1·05327	0·83194	0	6	−2	46
	16	1·19949	1·04147	0·83681	0	8	0	60
	17	1·20424	1·02915	0·84160	+2	9	0	74
	18	1·20877	1·01647	0·84612	5	8	+1	88
	19	1·21313	1·00321	0·85047	6	9	2	101
	20	1·21735	0·98952	0·85466	8	7	4	114
	21	1·22138	0·97516	0·85867	8	8	4	127
	22	1·22520	0·96019	0·86255	11	6	4	140
	23	1·22890	0·94464	0·86621	11	5	5	153
	24	1·23244	0·92829	0·86979	14	4	4	165
	25	1·23582	0·91129	0·87315	13	−1	4	177
	26	1·23907	0·89342	0·87634	13	0	6	190
	27	1·24216	0·87463	0·87946	14	+2	4	201
	28	1·24509	0·85489	0·88240	12	3	3	213
Mar.	1	1·24788	0·83399	0·88514	9	4	4	225
	2	1·25052	0·81199	0·88781	10	6	3	237
	3	1·25302	0·78856	0·89030	7	5	3	249
	4	1·25538	0·76377	0·89263	9	8	3	261
	5	1·25758	0·73725	0·89486	5	8	3	274
	6	1·25967	0·70884	0·89692	1	7	1	286
	7	1·26158	0·67827	0·89884	1	6	+1	300
	8	1·26335	0·64527	0·90062	2	6	0	314
	9	1·26498	0·60937	0·90227	+1	5	−1	328
	10	1·26647	0·56988	0·90380	−3	+2	2	343
	11	1·26781	0·52655	0·90513	4	0	2	357
	12	1·26901	0·47825	0·90632	3	−2	2	12
	13	1·27003	0·42374	0·90734	1	4	3	27
	14	1·27093	0·36134	0·90830	−1	5	2	42
	15	1·27168	0·28808	0·90903	+1	8	1	57
	16	1·27230	0·20030	0·90966	4	9	−1	71
	17	1·27276	0·09049	0·91007	6	9	+2	85
	18	1·27311	9·94306	0·91053	8	9	1	98
	19	1·27334	9·71853	0·91065	7	8	3	111
	20	1·27345	+9·22531	0·91078	11	8	2	124
	21	−1·27342	−9·26524	−0·91072	+12	−6	+3	137

Aberration Day Numbers—continued.

	log A.	log B	log *i*.	ΔA.	ΔB.	Δ*i*.	☾.
Mar. 22	−1·27326	−9·73022	−0·91059	+12	−4	+4	150
23	1·27297	9·94902	0·91029	13	2	6	162
24	1·27259	0·09367	0·90988	13	−1	6	174
25	1·27205	0·20199	0·90936	15	+1	4	186
26	1·27143	0·28856	0·90871	12	2	5	198
27	1·27065	0·36035	0·90792	12	4	5	210
28	1·26976	0·42190	0·90701	10	5	5	222
29	1·26876	0·47573	0·90592	10	5	5	234
30	1·26760	0·52350	0·90486	9	7	4	245
31	−1·26635	−0·56641	−0·90361	+6	+7	+3	257

Tables for the rest of the year have been communicated by the author, and are in the possession of the Society. They can be examined by anyone interested, and will be printed if a desire to that effect is expressed to the Secretaries.—[Editors.]

A Spectroscopic Determination of the Systematic Motions of the Stars. By S. S. Hough, M.A., F.R.S., and J. Halm, Ph.D.

The present paper contains a provisional discussion of the results, so far secured, of radial velocities of a catalogue of 318 stars brighter than 4·5 magnitude, and situated between the 30th northern parallel and the South pole.

After the completion of the investigation of solar parallax by velocities in the line of sight early in 1908, the catalogue was compiled for immediate observation, with the distinct view of thereby contributing towards our knowledge of the systematic motions of the stars relatively to our system by an extension to the southern heavens of the work already achieved by the Lick observers. The question of the "Sun's motion in space" has always been in the foreground of astronomical interest, but its importance for stellar physics has doubtless been considerably enhanced during recent years by the researches of Kapteyn, Eddington, and Dyson, who have established the existence of two distinct drifts of stars endowed with apparent systematic motions in different directions.

At first it might appear beyond hope to obtain any information relative to the two-drifts hypothesis by a study of radial velocity phenomena. It might be anticipated that the indications which would be separately furnished by each drift, if its constituent stars could be independently treated, would, from the combined material, merely indicate phenomena of an exactly similar character to those

resulting from a solar motion relative to a chaotic assemblage of stars.

From this reasoning all that seemed to be attainable from the work was a more secure determination, especially in declination, of the resultant vertex of these two drifts than could be obtained from northern stars only on account of their unsymmetric distribution over the sphere. It was with this object in view that the velocities of 166 stars of our list, which had so far been observed and reduced, were submitted to a preliminary discussion. To our surprise it was found that the material, when used in conjunction with Campbell's observations in the northern hemisphere, revealed with tolerable certainty some facts which, if true, appeared to be irreconcilable with the simple assumption that the Sun is moving in a chaos, but rather pointed to the existence of some subsidiary cause affecting systematically the motions of the stars utilised in the investigation. The discovery of this new phenomenon, presently to be described, was sufficiently tempting to undertake a more thorough analysis of the facts, which has led to conclusions in harmony with the two-drift hypothesis both in regard to the direction of motion and the distribution of the stars constituting the two drifts.

With regard to the material, the data published in Tables I., II. and III. supply all the information required in our investigation. In accordance with Campbell's arrangement (see *Astrophysical Journal*, vol. xiii. p. 80), two or more neighbouring stars were combined into groups, and the tables supply the mean right ascensions and declinations of these groups, their mean radial velocities, and the number of stars combined. The probable error of the observed velocities, so far as it is due to errors of observations and measurements, is about $\pm 0{\cdot}5$ km., a quantity which is certainly negligible if compared with the p.e. ± 12 km. which arises from the *motus peculiares* of the stars.

The shortness of time over which the observations extend, and the fact that for about 50 per cent. of the stars the velocities are based on single observations, make it impossible to take account of variability of radial velocity due to orbital motion. In such cases where the variability had been established by other observers, the stars were of course rejected. But although the material on which this research is based will doubtless be subject to modifications owing to the defect just mentioned, the results can be but little affected by errors of this nature. As positive errors due to orbital motion are as likely to happen as negative ones, we may assign to them the character of *motus peculiares*. Their only effect would therefore be to increase the probable errors of the result. But our calculations show that the p.e. of a single star of our list is not only not greater than that of a star in the Lick programme, but even sensibly less (about 11 km. as compared with 13 km.).

To the 166 stars observed at this observatory were added 45 stars whose radial velocities were known from other observations. Fifteen of these lying within the limits of declination of our list

were taken from Frost and Adams' paper on the radial velocities of 20 stars of the Orion type in *Publications of Yerkes Observatory* (vol. ii.); 13 are the motions of the centres of gravity of binaries whose orbits have been determined. The remaining 18 stars have been collected from publications in the *Astrophysical Journal*. The details of these 45 stars, collected into 25 groups, are given in Table II. Lastly, Table III. contains the data of the northern star-groups used by Campbell. It is reprinted from the above-mentioned publication in vol. xiii. of the *Astrophysical Journal*. We have thus 211 stars comprised within the parallel $+30°$ declination and the South pole, and 280 stars within the parallel $-30°$ declination and the North pole. The two sets overlap between $\pm 30°$ declination, and within that zone may have a number of stars in common. As Campbell's publication contains no list of individual stars it is impossible to fix the coincidences, the result being that the equatorial stars have a somewhat greater weight than the others. On the whole, however, we may say that the stars are fairly symmetrically distributed over the sphere.

TABLE I.

Groups of Radial Velocities of Southern Stars observed at the Royal Observatory, Cape of Good Hope.

Mean R.A.	Mean Decl.	Mean Observed Velocity.	No. of Stars.	Mean R.A.	Mean Decl.	Mean Observed Velocity.	No. of Stars.
h	°	km.		h	°	km.	
0·45	−53·6	+21·5	4	7·39	−70·0	+22·3	3
0·53	− 7·0	+21·7	3	7·75	+ 8·5	+ 6·9	4
1·27	− 1·5	+14·3	3	7·90	−23·8	+20·4	3
1·57	−48·5	− 0·1	3	8·15	−37·6	+ 4·3	3
2·60	+ 5·8	−19·6	3	8·37	−62·5	+20·0	2
2·87	−38·9	+32 6	3	8·75	+ 6·6	+30·9	2
3·60	− 9·6	+14·7	4	8·92	−37·1	+ 7·4	3
4·20	−52·6	+28·0	2	9·15	−54·1	+11·4	3
4·46	−31·7	+20·9	3	9·48	− 4·5	+ 8·7	2
4·60	+ 2·9	+40·8	3	9·67	−55·8	+ 6·3	3
5·04	−37·8	− 4·4	3	9·68	+20·3	+13·3	3
5·32	−12·1	+25·2	5	10·23	− 2·6	+ 7·4	3
5·75	−54·4	+32·5	4	10·28	−62·9	+ 4·5	3
5·79	−12·6	+28·1	4	10·53	−39·8	+ 9·5	2
6·20	−41·0	+13·4	4	10·93	−58·2	+ 6·3	2
6·80	+22·9	+ 8·6	2	11·15	−19·8	+ 8·0	4
6·80	−52·0	+26·7	2	11·88	+ 5·8	−13·6	2
6·81	−23·8	+18·2	3	12·07	−23·2	+ 6·2	2
7·28	−24·2	+45·2	2	12·53	−62·5	+19·1	3
7·33	−40·0	+54·7	2	13·60	+16·7	− 5·3	3

TABLE I.—*Groups of Radial Velocities*—continued.

Mean R.A.	Mean Decl.	Mean Observed Velocity.	No. of Stars.	Mean R.A.	Mean Decl.	Mean Observed Velocity.	No. of Stars.
h	°	km.		h	°	km.	
13·63	−29·3	− 1·7	2	18·08	− 6·2	+14·0	2
14·65	−52·3	−10·5	3	18·20	−28·7	−14·3	3
14·80	−29·8	−17·2	2	18·97	−21·2	−14·6	2
15·45	−37·4	−20·1	3	20·01	− 5·1	− 8·8	2
15·52	−21·1	−27·0	2	20·65	−53·2	− 2·9	2
15·90	+ 1·6	− 9·6	2	21·35	−15·3	− 8·1	3
16·37	−66·1	− 5·5	2	21·75	− 2·8	− 7·1	3
16·55	−30·2	− 2·9	2	22·64	−54·7	+19·2	3
17·04	+13·2	−18·8	2	23·20	−25·2	+ 8·5	3
17·55	−44·2	+ 0·1	3	23·55	+ 4·5	− 2·8	2

TABLE II.

Additional Groups of Radial Velocities obtained from Various Sources.

Mean R.A.	Mean Decl.	Mean Observed Velocity.	No. of Stars.
h	°	km.	
0·95	+ 1·9	+ 6·4	2
2·42	+30·5	+ 4·6	2
2·57	−54·5	+41·5	2
3·80	+31·6	+22·1	1
4·52	− 1·8	+14·0	2
5·22	− 5·2	+23·6	3
5·33	+ 2·3	+21·8	3
5·53	− 3·1	+22·1	3
5·53	+21·1	+16·4	1
6·02	− 1·3	+14·6	2
6·57	−23·4	+29·3	3
7·62	−41·7	+57·5	2
9·23	−56·6	+22·6	2
9·53	+14·2	+14·9	2
10·10	−11·9	+20·4	1
12·22	− 8·6	− 3·7	2
17·87	− 1·6	− 4·0	2
18·62	+29·9	−10·0	2
18·67	− 7·0	−34·5	2
20·13	+ 9·8	−22·5	2
20·30	−57·0	+ 2·0	1
20·78	+27·9	− 1·3	1
22·33	+27·3	0·0	2

TABLE III.

Groups of Radial Velocities of Northern Stars observed at the Lick Observatory.

Mean R.A.		Mean Decl.	Mean Observed Velocity.	No. of Stars.	Mean R.A.		Mean Decl.	Mean Observed Velocity.	No. of Stars.
h	m	°	km.		h	m	°	km.	
0	26·5	−14·0	+17·0	2	12	20·4	+43·0	− 4·7	3
0	40·0	+57·8	−10·3	6	12	35·7	−22·5	− 1·7	3
0	45·0	+28·2	−24·2	5	12	48·1	+ 4·9	−17·0	3
1	23·0	−10·1	+12·7	3	13	6·2	+23·2	− 8·2	2
1	35·5	+43·9	+ 0·2	4	14	9·2	− 7·6	+ 3·0	2
1	43·7	+10·6	+ 7·3	3	14	14·9	+18·1	− 3·9	5
1	47·4	−19·0	+ 2·5	2	14	29·5	−25·6	+11·8	2
2	2·6	+24·2	−19·0	2	14	58·0	+29·6	−13·1	7
2	38·5	−14·1	− 3·2	9	15	2·4	+46·0	−28·5	4
2	54·4	+ 0·1	+21·7	3	15	23·2	+ 4·8	− 7·0	4
2	55·4	+50·2	+13·6	5	15	27·4	+65·8	−14·2	4
3	3·8	+41·2	+ 4·5	4	15	41·4	−12·9	− 1·0	4
3	16·9	+13·6	+ 6·7	3	16	17·1	−20·8	−11·1	4
3	45·8	− 9·2	−10·7	3	16	32·8	− 8·5	− 7·0	4
4	3·2	−17·3	+34·8	3	16	38·0	+13·3	−26·0	3
4	16·6	+18·0	+35·5	5	16	49·5	+35·9	−29·0	3
4	29·7	+79·8	− 3·0	2	17	44·7	+53·6	−22·3	3
4	46·8	+44·7	+ 8·2	4	17	53·6	+29·5	− 9·9	4
5	15·7	+37·4	+30·7	3	17	53·8	+ 5·3	− 7·5	3
5	17·0	+ 7·1	+22·0	2	18	18·6	− 7·2	− 5·4	6
5	23·5	−20·9	+ 1·8	4	18	36·4	−23·5	−23·5	6
5	32·6	− 7·5	− 0·7	3	18	38·6	−19·0	−28·5	3
5	51·5	+57·7	+ 5·0	3	18	43·0	+41·2	−20·5	2
6	18·1	+25·0	+24·8	4	18	48·9	+69·6	+ 6·4	6
6	41·0	−15·6	+50·5	2	19	42·1	+ 6·2	−19·9	4
6	43·3	+16·7	+ 6·3	3	19	42·6	+23·6	−11·6	5
7	32·4	+27·0	+11·3	3	20	3·6	+55·4	−44·2	4
7	33·8	−25·1	+42·0	2	20	19·3	− 9·1	− 8·7	3
7	47·0	+ 9·3	+34·0	2	20	48·9	+43·3	+ 1·0	3
7	50·6	− 6·0	+21·0	2	20	58·5	+12·8	−28·2	5
8	56·6	+10·5	+26·9	4	21	1·3	+31·9	− 2·6	5
8	57·8	+32·0	+25·2	2	21	24·6	−15·8	− 2·9	4
8	58·0	+65·7	− 4·0	4	22	00·2	+12·7	− 7·3	3
9	10·2	+47·2	+19·0	2	22	13·6	+54·7	−14·2	2
9	27·2	− 3·7	+12·3	3	22	32·6	− 6·7	−11·3	4
9	57·2	+22·3	−15·2	2	22	52·9	+25·4	− 0·2	5
10	40·2	−15·2	+19·6	5	23	10·6	+71·4	−26·5	2
10	44·7	+38·1	− 2·8	5	23	10·7	−18·7	+ 5·6	4
11	11·6	+66·1	± 0·0	2	23	16·0	+43·8	− 2·0	2
11	32·8	+ 6·2	+ 3·7	6	23	31·0	+ 5·0	− 0·9	4

If V_0 denotes the velocity relatively to the Sun of a star of R.A. and Decl. α and δ directed towards a point A of the heavens whose R.A. and Decl. are α_0 and δ_0, V the component of this velocity in the direction of the line joining star and Sun, then we may write—

$$V = V_0(lx + my + nz) \quad . \quad . \quad . \quad . \quad (1)$$

where

$$l = \cos\delta_0 \cos\alpha_0 \qquad x = \cos\delta\cos\alpha$$
$$m = \cos\delta_0 \sin\alpha_0 \qquad y = \cos\delta\sin\alpha$$
$$n = \sin\delta_0 \qquad z = \sin\delta$$

V_0 may be due either to a motion of the star or to a motion of our system, or to both motions combined. Only in the case of all stars being either at rest or moving in random directions, *i.e.* without any common predilection for one certain direction, could we define V_0 as solar motion pure and simple. If all the stars, in addition to their chance motions or *motus peculiares*, have a common systematic motion towards a point A_1, whereas our system moves towards a point A_2, then the direction cosines l, m, n will determine neither A_1 nor A_2, but refer to a point A which defines the direction of motion of the one system relative to the other. This is obviously the most general form in which the one-drift theory may be expressed. It is the assumption on which the older methods of determining the position of the point A are based (Argelander, Airy).

A differentiation between the systematic motion of the stars and that of our system is impossible, unless there are distinguishing features in the arrangement of the masses, or systematic motions of the drift.

In the two-drift hypothesis advocated by Kapteyn, Eddington, and Dyson, instead of having only one resultant apex A, we have to consider two such apices A and A′, the first defining the direction of motion of drift I., the second the direction of motion of drift II., relative to our system. That means that in any part of the sky we have to deal with a mixture of stars partly consisting of the first and partly of the second drift. If the two drifts could be separated, our equation (1) would of course lead separately to a determination of A and A′.

The radial velocities of stars of the separate drifts would satisfy respectively the conditions

$$V = V_0(lx + my + nz)$$
$$V = V'_0(l'x + m'y + n'z),$$

whereas the actual observed radial velocity derived from small groups of stars should be regarded as representing values derived with weights proportional to the number of stars in the separate drifts. Thus if we suppose the number of stars in the first drift

bears on an average to those in the second the proportion $1-s:s$, the observed radial velocity will be expressible in the form

$$V=[(1-s)V_0l+sV'_0l']x+[(1-s)V_0m+sV'_0m']y$$
$$+[(1-s)V_0n+sV'_0n']z=V_0[Lx+My+Nz]\ .\qquad(2)$$

The first step was to examine whether the observations of northern as well as southern stars were satisfied by equations of the form (2). For this purpose a method of least squares solution for the three unknowns V_0L, V_0M, and V_0N was effected for the southern stars, a similar calculation for the northern stars having already been made by Campbell. Assigning the weights of the groups in proportion to the number of stars contained, we find—

For stars of Table I. only:	For stars of Tables I. and II. combined:
km.	km.
$V_0L=+19{\cdot}3\pm1{\cdot}7$	$+20{\cdot}2\pm1{\cdot}4$
$V_0M=+\ 4{\cdot}4\pm2{\cdot}0$	$+\ 4{\cdot}1\pm1{\cdot}7$
$V_0N=-10{\cdot}8\pm1{\cdot}9$	$-11{\cdot}9\pm1{\cdot}8$

The result shows that the numerical values of the three constants are not materially altered by the inclusion of the 45 stars of Table III., but, as was to be expected, their weight is increased. If these values obtained from southern stars are now compared with Campbell's results of northern stars, viz.—

Stars of Table III. (Campbell):

km.

$$V_0L=+18{\cdot}5\pm1{\cdot}6$$
$$V_0M=-\ 2{\cdot}4\pm1{\cdot}6$$
$$V_0N=-\ 6{\cdot}8\pm2{\cdot}0,$$

we come to the conclusion that their agreement is not quite so satisfactory as the assigned p.e. would lead us to expect. The discrepancies between the two hemispheres, especially in the M and N terms, are certainly great enough to demand a decision of the question whether they are purely accidental or due to systematic deviations of our observations from the simple conditions expressed by equation (2).

As a first tentative step towards this examination the following method was adopted. From the *combined* evidence of northern and southern stars the most probable values of V_0L, V_0M, and V_0N were derived, and with these the radial velocities for each group were computed and subtracted from the observed velocities. The residuals thus obtained were then grouped in order of right ascension by forming weighted mean residuals of all the stars between 0^h–2^h, 2^h–4^h, etc. This was done separately for the stars of Table I. and II. on the one hand, and of Table III. on the other. We thus obtain for each hemisphere mean residuals approximately corresponding to the hours 1, 3, 5, . . . 23. These are exhibited in the second vertical columns of Table IV. The

character of the curves, of which these residuals are the ordinates is, however, more emphasised if we further reduce the accidental errors by combining every three neighbouring values into means. These "smoothed" values are shown in the third columns, and are graphically exhibited in fig. 1.

TABLE IV.

Residuals Arranged in Order of Right Ascension.

Northern Hemisphere.			Southern Hemisphere.		
h h	km.	km.	h h	km.	km.
0– 2	−5·4	−4·3	0– 2	+6·2	+4·6
2– 4	−8·0	−3·4	2– 4	+0·9	+3·8
4– 6	+3·2	+1·5	4– 6	+4·4	+3·7
6– 8	+9·2	+5·9	6– 8	+5·8	+3·4
8–10	+5·4	+6·3	8–10	−0·1	+0·8
10–12	+4·4	+3·1	10–12	−3·3	−1·1
12–14	−0·6	+2 9	12–14	+0·2	−3·9
14–16	+4·8	+2·3	14–16	−8·5	−1·8
16–18	+2·6	+4·2	16–18	+3·0	+0·3
18–20	+5·2	+3·9	18–20	+6·5	+4·7
20–22	+3·9	+3·2	20–22	+4·5	+5·9
22– 0	+0·4	−0·4	22– 0	+6·6	+5·8

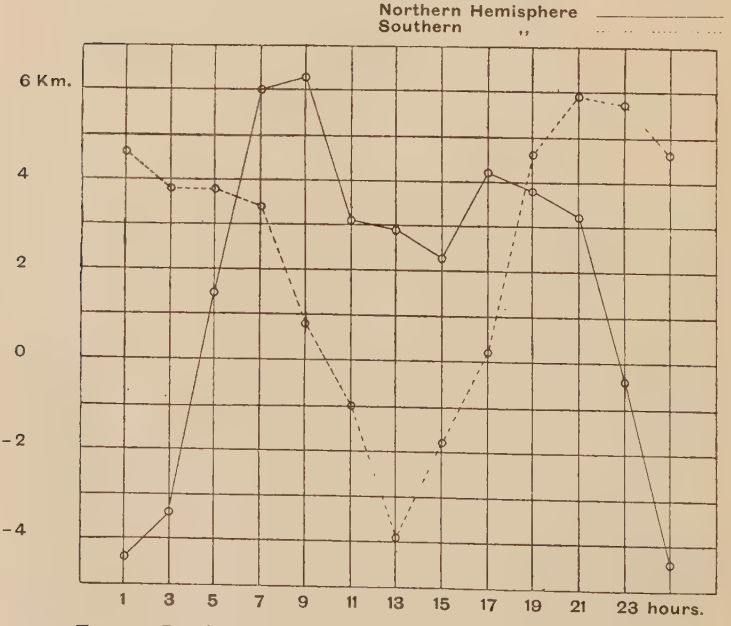


FIG. 1.—Residual Radial Velocities, distributed according to Right Ascension.

The impression gained from these curves cannot but point to the conclusion that the radial velocities of northern as well as southern stars are not completely represented by equation (2), but that, on the contrary, there is distinct evidence of systematic deviations depending on the position of the stars in the heavens. It will, of course, be noticed that the curves for northern and southern stars become exactly alike if the one is displaced twelve hours relative to the other. This fact suggests the existence of a single-periodic term with reversed sign north and south of the equator. In addition to this there appears a double-periodic term with maxima at about 8^h and 20^h, and minima at 2^h and 14^h, and this term does not reverse sign. Thirdly, there is evidence of a positive constant term shown by the fact that both curves are strongly displaced towards the upper side of the zero line. These features are very plainly brought to light if we form for each hour the quantities $\frac{N+S}{2}$ and $\frac{N-S}{2}$. The former will then exhibit the combined effect of the constant term and the term of double period, the latter the term of single period.

Diagram II. shows the corresponding curves.

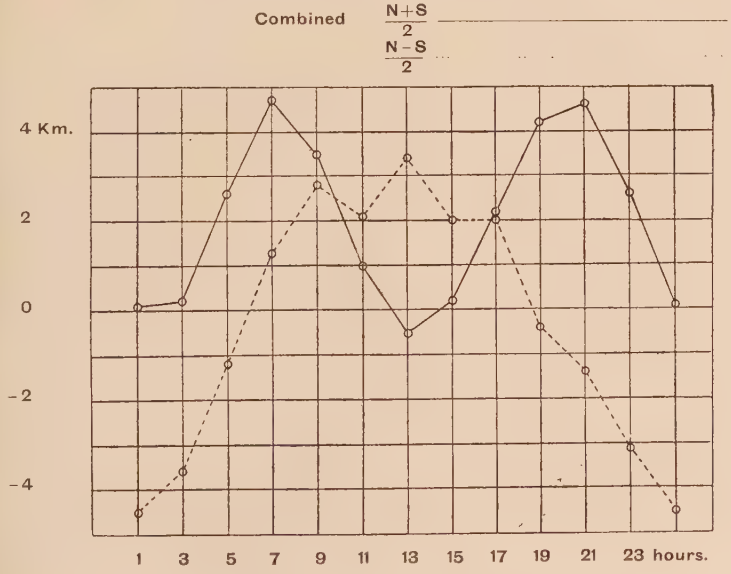


Fig. 2.—Residuals of both Hemispheres combined.

The systematic character of these phenomena is so clearly pronounced that we may venture even upon a further subdivision of our material with reference to declination. The stars were classified into three zones; the first zone including all stars north of $+30^\circ$ declination, the second zone extending from $+30^\circ$ to

$-30°$, and the third zone from $-30°$ to the south pole. In each of these zones the residuals were combined in R.A. as before, the mean residuals being formed by approximately the same number of stars. The results of this combination are shown in the following

TABLE V.

Stars between $+30°$ Decl. and North Pole.		Stars between $+30°$ and $-30°$ Declination.		Stars between $-30°$ Decl. and South Pole.	
h.	km.	h.	km.	h.	km.
1·5	−10·1	1·5	−1·5	1·9	+6·6
3·8	+ 5·5	3·7	+2·1	5·8	+4·5
7·1	+ 7·8	5·3	+2·9	8·4	+2·4
11·1	+ 1·1	7·4	+5·4	10·3	−4·3
17·0	+ 5·1	10·0	+4·1	14·8	−6·0
19·6	+ 9·6	12·4	−1·2	17·7	−1·8
22·8	+ 3·5	14·9	+7·7	22·0	+7·0
		16·7	+7·0		
		19·0	+6·1		
		22·1	−1·5		

Although the material on which these figures rest is even less certain than that previously discussed, the characteristic features of the three terms are clearly brought out. The equatorial zone comprising fairly equal numbers of stars north and south, shows no evidence of the single-period term, but exhibits the constant and double-periodic term very markedly, whereas in the northern and southern zones—in addition to these two terms—the influence of the term of single period with changed sign is clearly noticeable.

The simplest hypothesis which would serve to coordinate these phenomena would appear to be some hypothesis involving symmetry round an axis other than the polar axis of the Earth's rotation. Any such symmetrical distribution would give rise to periodic terms of the character found. To test whether such is the case, it was sought to express the residual velocities by means of six unknowns, a, b, c, d, e, f, such that the residual velocities Δv are expressible in the form—

$$\Delta v = a + b(\tfrac{2}{3} - \cos^2\delta) + c \sin 2\delta \sin \alpha + d \sin 2\delta \cos \alpha + e \cos^2\delta \sin 2\alpha + f \cos^2\delta \cos 2\alpha$$

$$= a + b\frac{2z^2 - x^2 - y^2}{3} + 2cyz + 2dzx + 2exy + f(x^2 - y_2).$$

This form is suggested as the most general by means of which the variations of velocity can be expressed by harmonic terms of order not higher than the second.

Introducing further the three unknowns g, h, i to represent the components of solar motion relatively to the mean distribution,

the equations of condition derivable from each group took the form—

$$a + b\frac{2z^2 - x^2 - y^2}{3} + 2cyz + 2dzx + 2exy + f(x^2 - y^2) + gx + hy + iz = V,$$

where V denotes the observed mean radial velocity of the group.

Solving these equations by least squares with weights proportional to the number of stars contained within the group we derived the following values for the nine unknowns—

$a = +2{\cdot}1 \pm 0{\cdot}4$	$d = -2{\cdot}1 \pm 1{\cdot}0$	$g = -9{\cdot}0 \pm 0{\cdot}9$
$b = +1{\cdot}9 \pm 1{\cdot}7$	$e = -2{\cdot}5 \pm 0{\cdot}8$	$h = -0{\cdot}4 \pm 0{\cdot}8$
$c = +0{\cdot}6 \pm 0{\cdot}9$	$f = -2{\cdot}2 \pm 0{\cdot}8$	$i = +18{\cdot}8 \pm 0{\cdot}7$

—where the probable errors are those derived *a posteriori* from the least square solution.

Thus the residual phenomena are expressible in the form—

$$\Delta v = +2{\cdot}1 - 2{\cdot}83x^2 + 1{\cdot}57y^2 + 1{\cdot}27z^2 + 0{\cdot}12yz - 4{\cdot}20xz - 5{\cdot}00xy \quad . \quad (3)$$

The direction cosines x, y, z of the directions of maxima and minima are given by the equations—

$$\left.\begin{aligned} (-2{\cdot}83 - \lambda)x - 2{\cdot}5y - 2{\cdot}1z &= 0 \\ -2{\cdot}5x + (1{\cdot}57 - \lambda)y + 0{\cdot}6z &= 0 \\ -2{\cdot}1x + 0{\cdot}6y + (1{\cdot}27 - \lambda)z &= 0 \end{aligned}\right\} \quad . \quad . \quad (4)$$

—from which we derive the discriminating cubic

$$\lambda^3 - 17{\cdot}06\lambda + 13{\cdot}17 = 0,$$

with the three roots

$$\lambda_1 = -4{\cdot}47, \qquad \lambda_2 = +0{\cdot}80, \qquad \lambda_3 = +3{\cdot}67.$$

The direction-cosines of the corresponding directions may be found from (4) by inserting the derived values of λ in these equations, or expressing these directions by means of their right ascensions and declinations (A, D) we find—

λ	A.	D.
$-4{\cdot}47$	$20^\circ{\cdot}7$	$+17^\circ{\cdot}0$
$+0{\cdot}80$	$89^\circ{\cdot}0$	$-50^\circ{\cdot}6$
$+3{\cdot}67$	$302^\circ{\cdot}7$	$-34^\circ{\cdot}2$

Taking these three directions as axes of reference and denoting by X, Y, Z, the direction-cosines with reference to them of the

line whose direction-cosines referred to the original axes are x, y, z, the expression for Δv reduces to

$$\Delta v = 2{\cdot}1 - 4{\cdot}47X^2 + 0{\cdot}80Y^2 + 3{\cdot}67Z^2 .$$

This form indicates a pronounced minimum in the direction of the diameter directed to the point

$$A = 20^\circ{\cdot}9 \qquad D = +17^\circ{\cdot}0$$

and a pronounced maximum in the direction

$$A = 302^\circ{\cdot}7 \qquad D = -34^\circ{\cdot}2 ;$$

but if either of these directions be regarded as an approximate axis of symmetry, the eccentricity in the corresponding equatorial plane is at least comparable in magnitude with the corresponding inequalities with reference to the two sides of this plane.

It appears, then, that the data cannot be sufficiently satisfied by any hypothesis involving symmetry about an axis.

Moreover, the strong determination of the constant element which appears at first sight to indicate a general dilatation of the universe of stars with respect to the solar system is one of the most prominent features indicated by the analysis. Though possibly in part to be accounted for by unknown systematic errors in the methods of determination, the amount, especially in consideration of the general agreement between the separate results obtainable from independent series of observations in the northern and southern hemispheres respectively, appears too large to be entirely attributable to such causes.

Reverting, however, to the two-drift hypothesis, it will be evident that the radial-velocity components indicated by the combined motions of the two drifts will only compound in such a manner as to indicate a determinate apex on the assumption, hitherto tacitly adopted, that the numbers of stars belonging to the separate drifts are proportionally the same throughout the sky.

If, on the other hand, in certain regions distributed systematically with reference to the sky as a whole the stars of one drift or the other relatively preponderate, the apparent phenomena resulting will be of a more complex character.

To examine the effects of the hypothesis of variable relative density of the stars constituting the two drifts in different regions, let us suppose that the quantity s introduced into equation (2) is no longer to be regarded as constant, but as a function of the direction (x, y, z). The simplest hypothesis to assume is that s is of the form—

$$S = S_0 + \epsilon(\alpha x + \beta y + \gamma z),$$

where $S_0 + \epsilon$, $S_0 - \epsilon$ denote respectively the maximum and minimum ratios of the density of the second drift to the star density as a whole, and α, β, γ denote the direction-cosines of the direction in which this maximum occurs.

The modified form of equation (2) will now be

$$V = [(1 - S_0)V_0 l + S_0 V'_0 l']x + [(1 - S_0)V_0 m + S_0 V'_0 m']y + [(1 - S_0)V_0 n + S_0 V'_0 n']z + \epsilon(ax + \beta y + \gamma z)[(V'_0 l' - V_0 l)x + (V'_0 m' - V_0 m)y + (V'_0 n' - V_0 n)z],$$

or if we put

$$\left.\begin{array}{l|l} (1 - S_0)V_0 l + S_0 V'_0 l' = V_1 L & V'_0 l' - V_0 l = V'_1 \xi \\ (1 - S_0)V_0 m + S_0 V'_0 m' = V_1 M & V'_0 m' - V_0 m = V'_1 \eta \\ (1 - S_0)V_0 n + S_0 V'_0 n' = V_1 N & V'_0 n' - V_0 n = V'_1 \zeta \end{array}\right\} \quad . \quad (5)$$

so that V'_0, V'_1 denote respectively the apparent resultant velocity of the Sun relatively to the two drifts, and the resultant relative velocity of the second drift with respect to the first, and L, M, N; ξ, η, ζ respectively the direction-cosines of these two velocities, the equation reduces to

$$V = V_1(Lx + My + Nz) + \epsilon V'_1(ax + \beta y + \gamma z)(\xi x + \eta y + \zeta z),$$

yielding an expression for V closely resembling in analytical form that derived from the analysis of our results.

On identifying this expression with that derived from our analysis we obtain

$$\left.\begin{array}{ll} \frac{1}{3}\epsilon V'_1(a\xi + \beta\eta + \gamma\zeta) & = a = +2{\cdot}1 \\ \frac{2}{3}\epsilon V'_1(2\gamma\zeta - a\xi - \beta\eta) & = b = +1{\cdot}9 \\ \frac{1}{2}\epsilon V'_1(\beta\zeta + \gamma\eta) & = c = +0{\cdot}6 \\ \frac{1}{2}\epsilon V'_1(\gamma\xi + a\zeta) & = d = -2{\cdot}1 \\ \frac{1}{2}\epsilon V'_1(a\eta + \beta\xi) & = e = -2{\cdot}5 \\ \frac{1}{2}\epsilon V'_1(a\xi + \beta\eta) & = f = -2{\cdot}2 \end{array}\right\} \quad . \quad (6)$$

These equations serve either for the determination of the three quantities

$$\epsilon V'_1\xi, \quad \epsilon V'_1\eta, \quad \epsilon V'_1\zeta$$

when a, β, γ are known, or for the determination of

$$\epsilon V'_1 a, \quad \epsilon V'_1\beta, \quad \epsilon V'_1\gamma$$

when ξ, η, ζ are known. In either case they take a linear form, and may be solved by the usual processes of least squares, provided appropriate weights are assigned. It seemed, however, preferable, instead of using the equations in this form, to take due account of the interdependence of the quantities a, b, c, d, e, f by a reversion to the original normal equations from which their numerical values were derived.

Suppose these normals were expressible in the form—

$$(AA)a + (AB)b + (AC)c + \ldots - (AN) = 0$$
$$(AB)a + (BB)b + (BC)c + \ldots - (BN) = 0$$
$$\ldots\ldots\ldots$$

and that the quantities a, b, c, etc. are eliminated in order after Gauss's manner.

It may readily be shown that in the usual notation appropriate to this method the sum of the squares of the residuals can be expressed as—

$$\frac{1}{(\overline{AA})}[(AA)a+(AB)b+(AC)c+\ldots\ldots-(AN)]^2$$

$$+\frac{1}{(\overline{BB_1})}[(BB_1)b+(BC_1)c+\ldots\ldots-(BN_1)]^2$$

$$+\frac{1}{(\overline{CC_2})}[(CC_2)c+\ldots\ldots-(CN_2)]^2$$

$$+\ldots\ldots\ldots\ldots$$

whence the condition that the sum of the squares of the original residuals should be a minimum is the same as that the sum of the squares of the quantities—

$$\frac{1}{\sqrt{(\overline{AA})}}[(AA)a+(AB)b+(AC)c+\ldots\ldots-(AN)]$$

$$\frac{1}{\sqrt{(\overline{BB_1})}}[(BB_1)b+(BC_1)c+\ldots\ldots-(BN_1)]$$

$$\frac{1}{\sqrt{(\overline{CC_2})}}[(CC_2)c+\ldots\ldots-(CN_2)]\quad\text{etc.}$$

should be a minimum. In other words, to effect any change of variables we may substitute for a, b, c, ... in terms of the new variables in the reduced normals and regard these as equations of condition, provided we attribute to the equations weights proportional to—

$$\frac{1}{\sqrt{(\overline{AA})}},\quad \frac{1}{\sqrt{(\overline{BB_1})}},\quad \frac{1}{\sqrt{(\overline{CC_2})}}\quad\text{etc.}$$

These reduced normals, in the order in which the elimination was effected, were as follows:—

$124{\cdot}9c$	$-\ 0{\cdot}1d$	$+\ 9{\cdot}3e$	$+\ 9{\cdot}6f$	$-\ 33{\cdot}0a$	$-\ 5{\cdot}5b$	$=-\ 25$
	$103{\cdot}9d$	$-\ 8{\cdot}1e$	$+\ 1{\cdot}4f$	$+\ 22{\cdot}4a$	$+\ 7{\cdot}0b$	$=-134$
		$149{\cdot}8e$	$-\ 7{\cdot}4f$	$+\ 8{\cdot}2a$	$-\ 0{\cdot}7b$	$=-338$
			$156{\cdot}2f$	$-\ 36{\cdot}8a$	$+\ 8{\cdot}3b$	$=-403$
				$464{\cdot}4a$	$-\ 38{\cdot}9b$	$=+913$
					$33{\cdot}0b$	$=+\ 61$

On reducing these equations to equal weight and substituting the expressions [6] for a, b, c, etc., we derive the following:—

$$\epsilon V'_1\xi(-0{\cdot}31\,\alpha+0{\cdot}41\,\beta) \quad +\epsilon V'_1\eta(0{\cdot}41\alpha-1{\cdot}16\beta+5{\cdot}56\,\gamma) \quad +\epsilon V'_1\zeta(5{\cdot}56\beta-1{\cdot}46\gamma)=-\ 2{\cdot}2$$

$$\epsilon V'_1\xi(+0{\cdot}46\alpha-0{\cdot}40\beta+5{\cdot}09\gamma) \quad +\epsilon V'_1\eta(-0{\cdot}40\alpha+0{\cdot}32\beta) \quad +\epsilon V'_1\zeta(5{\cdot}09\alpha+1{\cdot}42\gamma)=-13{\cdot}1$$

$$\epsilon V'_1\xi(-0{\cdot}05\alpha+6{\cdot}14\beta) \quad +\epsilon V'_1\eta(6{\cdot}14\alpha+0{\cdot}55\beta) \quad +\epsilon V'_1\zeta(0{\cdot}17\gamma)=-27{\cdot}7$$

$$\epsilon V'_1\xi(4{\cdot}93\alpha) \quad -\epsilon V'_1\eta(7{\cdot}55\beta) \quad +\epsilon V'_1\zeta(0{\cdot}32\gamma)=-32{\cdot}2$$

$$\epsilon V'_1\xi(8{\cdot}00\alpha) \quad +\epsilon V'_1\eta(8{\cdot}00\beta) \quad +\epsilon V'_1\zeta(5{\cdot}34\gamma)=+42{\cdot}0$$

$$\epsilon V'_1\xi(2{\cdot}87\alpha) \quad -\epsilon V'_1\eta(2{\cdot}87\beta) \quad +\epsilon V'_1\zeta(5{\cdot}74\gamma)=+10{\cdot}4$$

After assigning numerical values to α, β, γ, selected in a manner to be shortly described, these equations were combined by least squares, and the quantities $\epsilon V'_1\xi$, $\epsilon V'_1\eta$, $\epsilon V'_1\zeta$ derived. The process was then reversed, the derived numerical values of $\xi:\eta:\zeta$ being inserted in the equations, and the latter solved by least squares so as to obtain more approximate values of $\epsilon V'_1\alpha$, $\epsilon V'_1\beta$, $\epsilon V'_1\gamma$.

After three such approximations it appeared that the derived values represented the initial values with tolerable accuracy, and that no material advantage would be obtained by pushing the approximation further.

These values were as follows:—

$$\epsilon V'_1 = \ 7{\cdot}8$$

$$\alpha:\beta:\gamma = +0{\cdot}782 : -0{\cdot}575 : -0{\cdot}208$$

$$\xi:\eta:\zeta = -0{\cdot}030 : -0{\cdot}890 : -0{\cdot}454$$

—the sum of the squares of the residuals being reduced from 3853 to 560, while the corresponding values obtained for g, h, i are

$$\text{km.}$$
$$g = -\ 0{\cdot}4$$
$$h = +18{\cdot}8$$
$$i = -\ 9{\cdot}0$$

The complete expression for the radial velocity now derived is

$$\text{km.} \qquad \text{km.} \qquad \text{km.}$$
$$V = -0{\cdot}4\,x + 18{\cdot}8\,y - 9{\cdot}0\,z$$
$$\text{km.}$$
$$+\ 7{\cdot}8[0{\cdot}782\,x + -0{\cdot}575\,y - 0{\cdot}208\,z][-0{\cdot}030\,x - 0{\cdot}890\,y - 0{\cdot}454\,z].$$

The apparent velocity of the solar motion is $20{\cdot}85 \pm 0{\cdot}95$ km., directed to the point whose R.A. is $271^{\circ}{\cdot}2 \pm 3^{\circ}{\cdot}3$ and declination $+25^{\circ}{\cdot}6 \pm 3^{\circ}{\cdot}7$.

The direction of the relative motion of the two drifts is defined by the direction-cosines ξ, η, ζ, viz. the point whose R.A. is 268° and declination -27°, while the direction of maximum density of the second drift defined by the quantities α, β, γ is in R.A. 324° and declination -12°.

The product of the two quantities $\epsilon V'_1$ derivable from our data amounts to 7·8 km., but it is clearly impossible, on the evidence used, to determine the separate values of these two quantities.

To compare our results with those previously derived from proper motion investigations :—

An extensive investigation in connection with the Bradley stars has been published by Kobold (*Untersuchung der Eigenbewegungen des Auwers-Bradley-Catalogs nach der Bessel'schen Methode*, vol. lxiv. of the *Nova Acta der ksl. Leop.-Carol. deutschen Academie der Naturforscher*).

Though Kobold did not explicitly interpret his results as indicating two separate drifts of the stars concerned, he indicated certain stars, described as possessing retrograde motions, which did not fall in with his principal drift, and which, in the light of the later researches of Kapteyn, Eddington, and Dyson, might be regarded as constituting the members of a second drift.

From counts of stars contained in Kobold's catalogue in groups of R.A. the following table was derived :—

R.A.	Total Number of Stars.	Stars with Retrograde Motions.	Percentage of Retrograde p.m.
h h			
0– 2	153	57	37
2– 4	101	37	37
4– 6	107	24	23
6– 8	87	10	11
8–10	119	14	12
10–12	143	20	14
12–14	129	14	11
14–16	129	27	21
16–18	90	22	25
18–20	105	47	45
20–22	116	41	36
22– 0	134	48	36

In this table are quoted in the second column the total number of stars used by Kobold between the limits of R.A. given in the

first column. The number of stars possessing "retrograde" proper motion, *i.e.* those which may be presumed to belong to a second drift, are given in the third column, and the proportion of these to the total number of stars is given in the fourth column. It will be noticed that the number of such stars is proportionately least in R.A.'s about 10 hours, and greatest in R.A.'s about 22 hours. In fact the numbers in the fourth column may be very closely represented by the formula

$$25{\cdot}7[1 + 0{\cdot}58 \cos (\alpha + 26^\circ)].$$

Similar counts of stars grouped according to declinations failed to elicit any such systematic distribution.

It was accordingly assumed that the axis of greatest relative density of the second drift lies very approximately in the equator and in R.A. about -26°. This position was adopted as the initial position with which to commence the solution of our normal equations by the successive-approximation method described above.

The resulting derived values of $\xi : \eta : \zeta$, which indicate the directions of relative motion of the two drifts, is in very close agreement with that obtained by Dyson, viz.—

$$\left.\begin{aligned} A &= 268^\circ \\ D &= -27^\circ \end{aligned}\right\} \text{ from radial velocities.}$$

$$\left.\begin{aligned} A &= 268^\circ \\ D &= -23^\circ \end{aligned}\right\} \text{ from proper motions (Dyson).}$$

To determine the apices of each drift separately additional data are required. From the data derived from Kobold's investigation we may assign the value $0{\cdot}58 \times 0{\cdot}257$ to the quantity ϵ, whence we may derive the value

$$V'_1 = 52{\cdot}4 \text{ km.}$$

for the relative velocity of the two drifts.

Introducing the derived values into equations (5) we obtain

$$\begin{aligned} (1 - s_0)V_0 l + s_0 V'_0 l' &= -\ 0{\cdot}4 \\ (1 - s_0)V_0 m + s_0 V'_0 m' &= +\ 18{\cdot}8 \\ (1 - s_0)V_0 n + s_0 V'_0 n' &= -\ 9{\cdot}0 \\ V'_0 l' - V_0 l &= -\ 1{\cdot}6 \\ V'_0 m' - V_0 m &= -\ 46{\cdot}6 \\ V'_0 n' - V_0 n &= -\ 23{\cdot}8 \end{aligned}$$

km.

whence, on assigning different values to s_0, we obtain the following results :—

		km.		km.
$s_0 = \frac{1}{2}$;	$V_0 l$	$= -\ 0{\cdot}4$;	$V'_0 l'$	$= -\ 1{\cdot}2$
	$V_0 m$	$= +42{\cdot}1$;	$V'_0 m'$	$= -\ 4{\cdot}5$
	$V_0 n$	$= +\ 2{\cdot}9$;	$V'_0 n'$	$= -20{\cdot}9$
		km.		km.
$s_0 = \frac{1}{3}$;	$V_0 l$	$= +\ 0{\cdot}1$;	$V'_0 l'$	$= -\ 1{\cdot}5$
	$V_0 m$	$= +34{\cdot}3$;	$V'_0 m'$	$= -12{\cdot}3$
	$V_0 n$	$= -\ 1{\cdot}1$;	$V'_0 n'$	$= -24{\cdot}9$
		km.		km.
$s_0 = \frac{1}{4}$;	$V_0 l$	$=\ 0{\cdot}0$;	$V'_0 l'$	$= -\ 1{\cdot}6$
	$V_0 m$	$= +30{\cdot}4$;	$V'_0 m'$	$= -16{\cdot}2$
	$V_0 n$	$= -\ 3{\cdot}1$;	$V'_0 n'$	$= -26{\cdot}9$

We append the resulting determinations of the positions of the two apices, together with those derived from previous researches depending on proper motions alone :—

	Stream I.			Stream II.		
	V_1	A_1	D_1	V_2	A_2	D_2
	km.	°	°	km.	°	°
$s_0 = \frac{1}{2}$	42·1	90	+ 4	21·6	256	− 78
$s_0 = \frac{1}{3}$	34·4	90	− 1	27·8	263	− 64
$s_0 = \frac{1}{4}$	30·5	90	− 5	31·7	265	− 60
Kapteyn . . .	...	85	− 11	...	260	− 48
Eddington . . .	...	90	− 19	...	292	− 58
Dyson (1st paper) . .	...	94	− 7	...	240	− 74
„ (2nd paper) . .	...	93	− 7	...	246	− 64
Kobold	...	87	+ 3	...	...	...

Taking the mean of the results obtained by Kapteyn, Eddington, the two determinations by Dyson, and Kobold, the apex of Stream I. is

$$A\ +90^\circ \qquad D\ -\ 8^\circ,$$

that for Stream II. is

$$A\ \ 260^\circ \qquad D\ -61^\circ.$$

These values seem best reproduced by a somewhat smaller value of s_0 than that indicated either by Eddington or Dyson. At the same time it should be borne in mind that the material at present used is scanty, and too much significance must not be attached to the numerical results. The main feature is that the *discordances indicated in the radial motion investigation can apparently be completely reconciled with the phenomena deduced from the investigations of stellar proper motions by the additional hypothesis of a variability in the density of mixture of the two drifts, in a manner already exhibited by the data supplied in Kobold's work.*

A more exhaustive classification with regard to the spectral types of the stars or other such differentiating features, though in itself highly desirable, could not be undertaken with the material at present available. It is perhaps of some significance that the relative motion of the two streams is situated in the plane of the Galaxy, and directed towards its densest part.

Royal Observatory,
Cape of Good Hope,
1909 *October* 20.

Note by A. S. Eddington, B.A., M.Sc.

(*Communicated by request of the Secretaries.*)

In the discussion on the above paper, I called attention to a point in the interpretation of the data derived from Kobold's research, which the authors appear to have overlooked, and which appears to me to render the inferences from that part of the work unsound unless supported by a more minute investigation. The question is not a vital one, and the main investigation of the paper is practically unaffected; nevertheless, in view of the great importance of the results of the paper, it seemed desirable to the Secretaries that I should add this Note: it would have delayed the publication of the paper too long if I had first communicated with the authors and ascertained their views.

On p. 100, a table is given showing the proportion of Bradley stars (from Kobold's catalogue) having "retrograde motions" within various limits of R.A. The declinations of the stars range from about -25° to the North Pole. It is shown that this proportion is a maximum near R.A. 22^h and a minimum near R.A. 10^h; accordingly it is assumed that the axis of greatest relative density of the second drift lies approximately in R.A. 22^h. But the assumption that the proportion of retrograde motions measures even roughly the relative density of drift II. in the corresponding part of the sky is quite unfounded. Surely it is reasonable to attribute the small proportion of retrograde motions near R.A. 6^h, not to the fewness of drift II. stars, but to the fact (which is easily seen from a diagram) that *the motion of drift II. in most of that region of the sky is not retrograde.* Similarly, the high percentage near R.A. 18^h is due to the fact that there drift II. has its strongest retrograde motion. The point I wish to bring out is that, in interpreting the table referred to, it is essential to have regard to the variation in the aspect of the drifts, which is quite as important as a variation of density. I do not suggest that the alleged variation of density does not exist, for I attach great importance to the results deduced by the authors from the radial velocities; but without a more minute investigation Kobold's work cannot be said to support it.

If my own determination of the position of the drift II. apex

(R.A. $19\frac{1}{2}^h$) is used, although it does not actually agree with the point of maximum percentage of retrograde motions (R.A. 22^h), it is clear that a great part of the variation of percentage could be accounted for without assuming a variation of density. If, however, Dyson's determination (R.A. $16\frac{1}{2}^h$) is used the discordance is $5\frac{1}{2}^h$, and the variation of percentage is not explained; but it is not permissible therefore to say that the table indicates a maximum density of drift II. at 22^h. It is necessary to explain, not only why there is an unexpected periodic term with a maximum at 22^h, but also why the expected periodic term with maximum at $16\frac{1}{2}^h$ has been wiped out. The deduced direction of maximum of drift II. would thus differ considerably from 22^h.

The effect on the rest of the paper of eliminating the part objected to is not great. In particular, the determination of the maximum relative intensity of drift II. from the radial velocities is probably unaffected; also the determination of the vertex. The value $V'_1 = 52{\cdot}4$ km. must be rejected, but the authors themselves attach little importance to it; it could be replaced by an almost identical value inferred both from Dyson's and Eddington's determinations of the drift motions taken in conjunction with the known linear value of the solar motion.

As, however, it is of considerable importance to examine whether the authors' interpretation of their radial velocity results is supported by a discussion of the tranverse motions, perhaps I may be allowed to publish here the result of an examination of the proper motions of Zodiacal Stars which supports their hypothesis, and may perhaps in some degree replace the table which I criticised. When we compare one part of the sky with its exact antipodes, the motions are seen projected on the same plane, and a direct comparison is possible. We can thus infer which has the greater proportion of drift II. stars. Even where drift II. is not moving retrograde, its forward motion is less than that of drift I., at least in the regions here discussed, so that the majority of the retrograde motions belong to it. In the following table I compare the percentages of retrograde motions at opposite points of the ecliptic.

Zodiacal Stars.

Longitude of Region.	"Retrograde Motions" per cent.	Longitude of Region.	"Retrograde Motions" per cent.	Difference Column 4 − Column 2.
°		°		
0	30	180	28	− 2
22·5	36	202·5	23	−13
45	18	225	21	+ 3
67·5	11	247·5	20	+ 9
90	16	270	24	+ 8
112·5	11	292·5	16	+ 5
135	13	315	28	+15
157·5	28	337·5	33	+ 5

The actual numbers in the last column have not much significance; but the run of the signs shows that from longitude 214° to 34° drift II. consistently preponderates, agreeing well enough with Hough and Halm's determination, which fixes the maximum at R.A. 324°.

It should, however, be stated that a small systematic correction to the proper motions might considerably alter the above table, and I could not place confidence in it independently. Its sole value arises from the fact that it seems to agree so definitely with Hough and Halm's result.

Errata to vol. lxviii. of the Monthly Notices (*Prof. E. W. Brown's paper*).

p. 455, line 11, *for* "(1/296–3)" *read* "(1/292–9)."
,, footnote, *for* "(β)" *read* "(α)."
,, postscript, for a correction to this statement see *Monthly Notices*, vol. lxx. p. 3.